

JOURNAL
of the
Society for Psychical Research

VOLUME 37 No. 676 July-October 1953
(Double Number)

ANTOINE RICHARD'S GARDEN
A POSTSCRIPT TO 'AN ADVENTURE'

BY G. W. LAMBERT, C.B.

Introductory Note

PART I of the following paper was written in response to an invitation from the Editor of the *Journal* to review an article in a French periodical, referred to below. In order to carry out that task, it was necessary to re-examine carefully the evidence in *An Adventure* on which the article was based. That investigation, in turn, led me to make some historical inquiries which have opened up a new line of interpretation of the evidence. Part II of the paper gives the results of the investigation so far obtained. They are sufficiently curious to be very thought-provoking.

PART I—THE JEU DE BAGUE

In a recent article in the *Revue de Paris*,¹ under the title of 'Une Promenade hors du Temps', M. Léon Rey, the learned author of *Le Petit Trianon*,² recalls the strange narratives of the late Miss C. A. E. Moberly and the late Miss E. F. Jourdain, describing their walk in the Trianon Gardens on 10 August 1901 during which, at certain places, they saw persons and scenery which they afterwards attributed to the period of Marie Antoinette, and in particular to the events of the year 1789. The story was first published in 1911, under the title of *An Adventure* (Macmillan), pseudonyms being used, Miss Moberly being 'Miss Morison' and Miss Jourdain being 'Miss Lamont'. The 1911 edition was reviewed at length in the Society's *Proceedings* (XXV, 353–60).

¹ December 1952, pp. 117–23.

² *Le Petit Trianon et le Hameau de Marie-Antoinette*, par Léon Rey, Archiviste Paléographe; Pierre Worms Ed., 1936, pp. 84, illus.

The reviewer came to the conclusion that there were not sufficient grounds for assuming a paranormal explanation of the experience, and attributed some of the things 'seen' to faulty memory resulting from confused mental images of things actually seen. The case nevertheless remained a controversial one, and the book ran to four editions, the last of which has been reprinted four times. In this paper *An Adventure*, for the sake of brevity, is referred to as 'A', and, unless otherwise stated, page references are to pages of the *Fourth Edition* (Faber & Faber). The evidence has a complicated history, and a full critical appraisal of its value as evidence will be found in a Note by W. H. Salter in the Society's *Journal* for January-February 1950 (XXXV, 178-87).

Here it is only possible to sum up very briefly indeed what is a long and involved story. The two 'observers', as we may call them, did not *at the time* (10 August 1901) fully realize that some of the things they had seen and heard were not normal, and the earliest surviving written records were not committed to paper till about three months after (November 1901). The observers, moreover, tried to verify their experiences by historical research, conducted at irregular intervals over a period of years, a process which, as W. H. Salter points out in his Note referred to above, has plunged the evidence into a fog of uncertainty, making it impossible to distinguish features of the original experiences from features, acquired by later research, 'read back' into it, and unconsciously colouring their memories of the former. In 1938 a book entitled *The Mystery of Versailles*¹ was published, with a foreword by the late Harry Price. The author, Mr Sturge-Whiting, endeavoured to explain away the 'mystery' by attributing it wholly to malobservation, defective memory, and unconscious self-deception in the course of research. The attempt cannot be regarded as entirely successful, as the author, in criticizing the two observers for special pleading, himself resorts to special pleading on a number of points to a degree which vitiates many of his arguments. He makes some useful observations, but his general conclusion is far too sweeping. Since 1938, brief references to A were made by the late G. N. M. Tyrrell in his 1942 Myers Memorial Lecture *Apparitions*,² two of which are quoted below; and in 1950 W. H. W. Sabine discussed the case at greater length in the *Journal of the American Society for Psychical Research*, in the course of an article entitled 'Is there a Case

¹ J. R. Sturge-Whiting, *The Mystery of Versailles : a complete solution*. London, Rider, 1938.

² The edition published by the S.P.R. in 1943 is out of print. A new edition will be published by Duckworth in the autumn of 1953.

for Retrocognition?" (XLIV, 48-52). Both these authors accepted the view that the two ladies were hallucinated, but drew different conclusions from the evidence. Whatever view the reader of this paper may have formed about *An Adventure* up to now, he is faced with a new problem as the result of M. Rey's article in the *Revue de Paris*, to which we now return.

M. Rey takes, on the whole, a conservative view, and criticizes the two observers for overstraining the evidence they acquired by research, and forcing it, in an inadmissible manner, to support theories of identification which they had built upon insufficient grounds. The main object of his article, however, is to bring to notice a 'small discovery' he has made concerning the possible identity of *one* of the 'visionary objects' seen by both the observers on 10 August 1901 in the English Garden, namely the 'kiosk', which was afterwards found not to have been there at the time of their experience (A, p. 73). As a result of subsequent research, they eventually identified the 'kiosk' as a sham 'ruin' of seven Ionic columns, walls and a domed roof, of which they found in 1908 an undated design among the French Archives relating to the Petit Trianon. The evidence, as described on p. 23 of A, on which the ladies relied to show that a 'ruin' *to that design* was ever actually built at or near the spot where they saw the 'kiosk', is wholly unconvincing, and their reasoning is a notable example of the tendency, even among persons with a historical sense, 'to find what they are looking for.' M. Rey points out that the design mentioned remained a design and was never actually built. That consideration by itself does not rule out the identification, because an 'object' in a phantasmal scene may not be related to a real original at all. It is a pure assumption that a particular phantasmal scene is exactly related to a past actual scene, and the onus of proving that it is so related rests upon the person seeking to show that it is. But we may nevertheless agree with M. Rey that the identification is inadmissible, on the more cogent ground that the design for the 'false ruin' does not answer to the description of the 'kiosk' given by the two observers. That design, we are told (A, p. 23), was taken from that of a ruin at Baalbek, shown on Plate 44 of *The Ruins of Baalbek* by R. Wood (London, 1827).¹ Miss Jourdain thought this plate resembled 'our kiosk. Both were round, had low walls, pillars and a roof with a slightly Chinese effect [Miss Jourdain's italics]

¹ The ruins of Baalbek were visited by Wood in 1751 and the design in question was no doubt available to architects and others soon after that date. The date 1737 assigned to Wood's book in Miss Moberly's note cited below (p. 143) is clearly an error.

in the upward curve of the roof' (A, p. 24). This is a very surprising judgment, as the building shown on Plate 44 of Wood's book is in late Classical style, and it could only by a considerable stretch of imagination be said to have a Chinese look about the roof.

M. Rey, on the other hand, seeks to identify the 'kiosk' with another building called the 'Jeu de Bague', a species of round-about, which was definitely in the Chinese style. It was built in 1776 and removed during the Revolution. A water-colour painting of this 'novelty' has survived, and is in the Parmentier Collection. A reproduction of that picture, in black and white, appears at the head of M. Rey's article, and a much clearer reproduction of it will be found in his book¹. The circular moving platform is sheltered by a round conical roof, supported by a central pillar. The only noticeable feature of the central portion is the roof, which is 'recurved', and runs up to a finial, surmounted by an ornament. At a short distance there is a semicircular gallery for spectators (added in 1782), with a roof in an even more pronounced Chinese style. M. Rey (p. 122 of his article) records the fact that the two observers had seen a picture of the Jeu de Bague in a bookseller's shop, but he seems not to have had in front of him the date of their seeing it, namely 4 July 1904. The two observers, while noticing some likeness between the 'central building' and the 'kiosk' (A, pp. 64 and 66), rejected the identification of the two. M. Rey suggests that they were so strongly attached to the hypothesis of a 'ruin' that 'they would not believe they had really seen the Jeu de Bague'. In making this identification, notwithstanding the observers' rejection of it, M. Rey hardly does justice to the reasons they gave for the rejection. They were as follows :

(1) ' . . . the surrounding part was not like' (A, p. 66). This probably refers to the semi-circular gallery, which was part of the whole edifice (after 1782). M. Rey has taken 'the surrounding part' to mean 'the surroundings', and has read the observation as referring to the absence of a screen of trees, which had not yet been planted. If the scene had been that shown in the picture, the two ladies, while approaching the House (as the Château was usually called) on 10 August 1901, could hardly have avoided seeing the Jeu de Bague on their right. As they did not see it, they assumed afterwards that it was screened by trees (see A, pp. 92-3).

(2) ' . . . its position was unsuitable for our purpose'. This consideration seems to have weighed heavily in the estimation of the two observers. The site of the Jeu de Bague was just inside the French Garden, and some appreciable distance (150

¹ Op. cit., p. 73.

metres approximately) from the place where the 'kiosk' was seen in the English Garden. It is therefore hardly possible to accept M. Rey's suggestion that 'at the place where they had seen a Chinese kiosk looking like a bandstand they had really seen a Jeu de Bague much like the kiosk they had described' (pp. 122-3 of his article). Further, it is difficult to believe that the hypothesis of the 'ruin' had taken such firm hold on their minds by July 1904, considering that it did not fully take shape until 1908, when they saw the picture of the Baalbek ruin.

While, then, one cannot follow M. Rey in identifying the kiosk with the Jeu de Bague, he is much to be congratulated on his acumen in seeing that, in this context, the adjective CHINOIS, which he has put in capital letters, is of crucial importance. It has turned out to be a key which has opened a door, not on to a 'complete solution' but on to a new mystery.

PART II

I. THE KIOSK

OWING to the state of the evidence, it is unfortunately necessary to start this section with a rather minute discussion of the question whether the recollection of the 'Chinese effect', as a feature of the kiosk, can be relied upon, or whether it is a 'false memory', acquired from something seen in later research. For this purpose one must turn to the second edition of *An Adventure* (published in 1913 and now rare), which contains in an Appendix verbatim copies of the two 1901 versions of the stories by each observer,¹ and a number of dated entries relating to later research. In the 1901 narratives there is no hint of a Chinese effect. From the four narratives taken together we learn the following particulars about the 'kiosk'.

General situation. 'It was on rough uneven ground, and was overshadowed by trees.' (In a section to which both observers appended their initials, A, Second edition, p. 48.)

General appearance. 'A light garden kiosk, circular, and like a small bandstand.' (Miss Moberly in M.1., *ibid.*, p. 194.)

Detailed features. 'A low surrounding wall' (*ibid.*, p. 48), 'steps' (pp. 190 and 202), a 'balustrade' (just above the steps) (pp. 185 and 190), 'pillars' or 'columns' (pp. 48 and 202), and a 'roof' (p. 202). It is not clear whether the low surrounding wall was the lower part of the building, or a separate wall enclosing a courtyard, in which the kiosk stood.

¹ These are the M.1 and J.1 and M.2 and J.2 of the Note by W. H. Salter cited above.

There is no hint of a 'Chinese effect' until July 1904 when, as related above, the two ladies revisited Versailles and saw, presumably for the first time, a picture of the Jeu de Bague in the shop of a bookseller there. 'He showed us a picture (which he would not part with) of the Jeu de Bague. We saw at once that the central building had some likeness to the kiosk, but the surrounding part was not like, and its position was unsuitable for our purpose.' (Statement initialled by both observers, *ibid.*, p. 38). The 'central building', as stated above, is little more than a circular conical roof to shelter the revolving platform of the 'round-about', and its 'mounts' of dragons and peacocks. From the illustration, as also mentioned above, it is clear that the roof (the only striking feature) was in the Chinese style. The Chinese look of the surrounding gallery is still more obvious. The ladies *at once* saw some likeness between the kiosk and the central building, but, for the reasons given, would not identify them. This suggests that the mental image of the kiosk which they both already had in July 1904, before seeing the picture, had a sufficiently Chinese look to make it resemble the central building, but was not sufficiently like in other respects to justify them in identifying the two. It was not until September 1908 that Miss Jourdain found in the French Archives 'a paper (without signature or date) giving the estimate for a "ruine" having seven Ionic columns, walls, and a dome roof' (*ibid.*, p. 48). It was the design of this 'ruine' which was taken from Wood's *Ruins of Baalbek*, mentioned above. When, after September 1908, Miss Jourdain saw Wood's Plate 44 is not stated, but it was when she compared it with her recollection of the kiosk that she said both had 'a roof with a slightly Chinese effect in the upward curve of the roof' (A, Fourth edition, p. 24).

In view of the late appearance of the 'Chinese look' in the story, must it be condemned as an unreliable recollection? I do not think so, for two reasons. In the first place, there is no obvious reason why it should have been dragged in, if not a genuine detail. Both observers were over-anxious to prove the historical accuracy of their 'visions', and the introduction of a Chinese effect, whether they realized it or not, greatly intensified for them the problem of identifying the kiosk. In the course of their walk round the Gardens in 1901 they saw nothing real to suggest a Chinese effect; much less had they reason later on to invent it, in order to make it easier for them to identify the kiosk. Secondly, the impression, as a feature of the mental image of the kiosk, seems to have been so strong, in 1908 or later, as to lead Miss Jourdain to attribute to the roof of the Baalbek ruin a

Chinese effect which is certainly not there. In other words, the mental image of the kiosk seems to have 'warped' her perception of the illustration in Wood's book, and not *vice versa*. It is therefore reasonable to allow the adjective 'Chinese' to stand, in the description of the kiosk. That building was not the Belvedere ; it was not the Temple de l'Amour (for pictures of these see *Proceedings*, XXV, 358) ; no composite mental image of these, as suggested by the reviewer of 1911, would produce a Chinese effect ; it was not the Baalbek ruin design ; and now we have had to confirm the observers' own conclusion that it was not the Jeu de Bague.

What, then, was the kiosk which the two ladies saw? As a result of studying M. Rey's article, I asked myself that question, and the conclusion to which I found myself led was both extraordinary and unexpected.

2. ANTOINE RICHARD'S GARDEN

In the circumstances, it is desirable to set down in order the stages of reasoning and research which led to the conclusion. The method used was that of following the argument wherever it might lead, without prejudging the outcome, instead of starting with a fixed idea that the kiosk was this or that known object, and then picking out those and only those bits of evidence which seemed to support the hypothesis.

To begin with, the reader is invited to turn back to the collection of descriptive details of the kiosk set out above. These enable one to form a fairly clear mental picture. To me they suggested very strongly a garden pavilion, put there for scenic effect rather than for use ; and, if one allows the roof to have a 'slightly Chinese effect', the building becomes in the mind's eye a Chinese garden pavilion. Near it, in the ladies' description, was a rustic bridge over a tiny ravine, and a small cascade. These three, the pavilion, the bridge, and the cascade, are the standard features of a Chinese Garden of a type very fashionable in the second half of the eighteenth century, both in England and in France. It was usually associated in France with the 'Jardin Anglais', of which it formed part. Was there ever a Chinese Garden in the north-west part of the English Garden at the Petit Trianon?

In trying to answer that question I obtained invaluable help from the Tableau Chronologique at the end of M. Rey's book (pp. 78-82). Earlier (p. 58) he had commented on the wonderful harmony among the ornamental works at the Petit Trianon, and it was difficult to believe that the discordant element of a Chinese Garden would have been allowed to exist for very long in the

close proximity of such works as the Belvedere (begun in 1778) or the Temple de l'Amour (begun in 1777). There was just a chance that one had been made about the same time as the Jeu de Baguette in 1776, and that it had been swept away a little later, without trace of it being left. In *The Story of the Garden* Eleanor Rohde, writing of this period, says: 'Marie Antoinette had probably only a vague idea what "le jardin anglais" was like; the King had given her the Trianon and she was determined to have a garden in the new style. An English gardener Richard by name was appointed and the grounds were soon adorned with a Chinese aviary, Turkish fountains and other follies. Marie Antoinette was a genuine flowerlover and these failed to please her, and Richard was dismissed.'¹ Was a Chinese pavilion standing for a year or two at or near the spot where the 'kiosk' was seen, and then swept away as one of the 'follies'? In 1774, before the arrival of the new architect Richard Mique (appointed 1775), the construction of a Chinese Garden, with a pavilion and rustic bridge mostly made of timber, might have been carried out by the head gardener, Antoine Richard, with the assistance of the chief carpenter. If Richard had designed a 'Chinese pavilion', what would it have looked like? To answer this question one needed to learn something about Antoine Richard and the sources from which he would have been likely to get a design. He belonged to a family of Irish emigré descent which had been employed at Versailles and the Trianon since the middle of the eighteenth century. In 1774, the year in which the Petit Trianon was completed, he submitted to Marie Antoinette a plan for an English garden there, but his plan was rejected in favour of one made by an amateur, M. de Caraman.² On 6 October 1774 he and his son were ordered to make the 'terrasses' of the new garden,³ presumably according to de Caraman's plan. From that time on for three or four years the work of constructing the English Garden, especially that part in which the 'kiosk' was seen, was proceeding so actively that it was hard to believe that any of old Richard's ideas ever materialized. With the help of M. Rey's *Tableau Chronologique* one could picture the upheavals in 1776,

¹ E. S. Rohde, *The Story of the Garden* (London, Medici Society, 1932). Richard was not dismissed. He died in the Queen's service in 1784 (Rey: *op. cit.*, p. 81).

² Th. Blaikie, *The Diary of a Scotch Gardener* (London, Routledge, 1931), p. 167 n.; and Richardson Wright, *The Story of Gardening*, (London, Routledge, 1934), p. 362.

³ Rey, *op. cit.*, p. 78. The son's name was Claude. He succeeded Antoine as gardener to the Queen on his death in 1784 (*ibid.*, p. 81). Footnote 1 in A, p. 78, is not quite accurate.

when the river was being excavated and the Jeu de Bague was being built. Then in 1778 the building of the Belvedere was started, with all the disturbance that involved in the way of carts going to and from the gardener's gate. There seemed to be little likelihood of a Chinese Garden having ever existed anywhere near the place where the two ladies saw the kiosk, the rustic bridge, and the cascade. Historically speaking, it seemed to be a case of a Chinese Garden having been made at the spot indicated, either in the short period 1774-6, or never at all.

Even so, it seemed worth speculating as to the kind of Chinese pavilion old Richard would have contemplated. The design of the Jeu de Bague was inspired by that of one in the garden of the Duc de Chartres at Monceau,¹ and there were probably Chinese details in the garden of de Caraman, whose taste Marie Antoinette admired so much that she took his plan for the Trianon garden, in preference to that of Richard.² Further, at this time it was the fashion among the French aristocracy to have not only a 'jardin anglais' but also an English or Scotch gardener, and Richard doubtless had English-speaking friends among the gardeners in and around Paris. He was also likely to have been in touch with English literature on the subject.

At this period the arbiter of taste in England, so far as the Chinese style was concerned, was Sir William Chambers,³ Architect to George III, who designed and built in Kew Gardens the pagoda (still standing), an aviary, and a pavilion in the Chinese style (no longer existing).⁴ His book *Designs of Chinese Buildings* etc., with numerous plates, published in London in 1757, both in English and in French, was followed up by *A Dissertation on Oriental Gardening* (no illustrations) in 1772. In his younger days Chambers had been to Canton, and had made careful drawings of Chinese buildings. I had no reason at that stage to suppose that Antoine Richard had actually seen Chambers's work, but it seemed likely that he had, especially as Chambers paid a visit to Paris in 1774. It certainly was an odd coincidence that the main features of the kiosk, as described by the two observers, seemed to combine those of two designs for garden pavilions in the Chinese style shown on Plate VI of Chambers's *Designs of Chinese Buildings* (see Fig. 2 of the present paper).

The work most likely to provide a link between Chambers and

¹ Rey, op. cit., p. 30, and Wright, op. cit., p. 365.

² Wright, op. cit., p. 366. ³ See D.N.B., Sir William Chambers.

⁴ Illustrations of these three buildings in Kew Gardens, from contemporary prints, will be found in Ralph Dutton, *The English Garden* (London, Batsford, 1937), Figs. 154-6.

Richard, if a link existed at all, was Le Rouge's *Collection des Jardins Anglo-chinois* published in Paris in Parts beginning in 1776. On consulting that voluminous work, which consists wholly of plates with no separate text, I discovered a document of whose existence I had no previous knowledge and which I certainly did not expect to find, namely Antoine Richard's *rejected* plan for the Petit Trianon. In Vol. I of the British Museum copy, Plate No. 19 of the 6th 'Cahier' bears the description 'Projet pour le Jardin Anglo-Chinois du Petit Trianon par Antoine Richard, Jardinier de la Reine, 1774'. It is a plan (see Fig. 3) showing a number against each principal feature, with a key at the side. It also has a scale in 'toises', an obsolete French word meaning fathoms.¹ The position of the Château being shown, the exact position of every detail in the plan can be calculated, and comparison can be made with earlier and later maps.

This 'Projet' is followed by a map (Plate 20) showing the Petit Trianon gardens as laid out by Louis XV as a Botanic Garden and by three plates (Nos. 21, 22, and 23) showing the elevations of some of the buildings included in the Projet. It is thus possible to judge the features visible at the beginning of 1774, just before the death of Louis XV, and to get a fairly accurate idea of the alterations which Antoine Richard's plan would have involved. With the assistance of the plan (Pl. 19) and the plates relating to it one can, by an exercise of the imagination, 'take a walk' in 'Richard's Garden', as we will call it, and observe the main features in their appropriate places. If the reader, for the sake of argument, will do this, he will be astonished, as I was, to find a kiosk (No. 7) close to where the two observers found one in August 1901, and a small bridge crossing a stream just above it ; secondly, a 'barrier' not far beyond it, from the east end of the (old) orangerie to the house, like the one which prevented them from seeing in that direction when they crossed the meadow visible on their left ; thirdly, a 'second house' having one end at right angles to the terrace giving on to the French Garden, like that which the observers saw (A, pp. 49 and 55), when they were approached by the so-called 'Chapel man' ; and, fourthly, north-east of the kiosk, a wood with winding paths, very like the one in which Miss Jourdain lost her way in 1902—all in their 'appropriate places'. These correspondences are so striking that it looks as if the two ladies had 'strolled through Richard's garden', so to speak, in a sort of dream. Taking the above-mentioned features one by one :

1. *The kiosk.* This feature, No. 7 on Richard's Plan, is almost

¹ 1 toise = 1.949 metres = 6.2 feet (approx.).

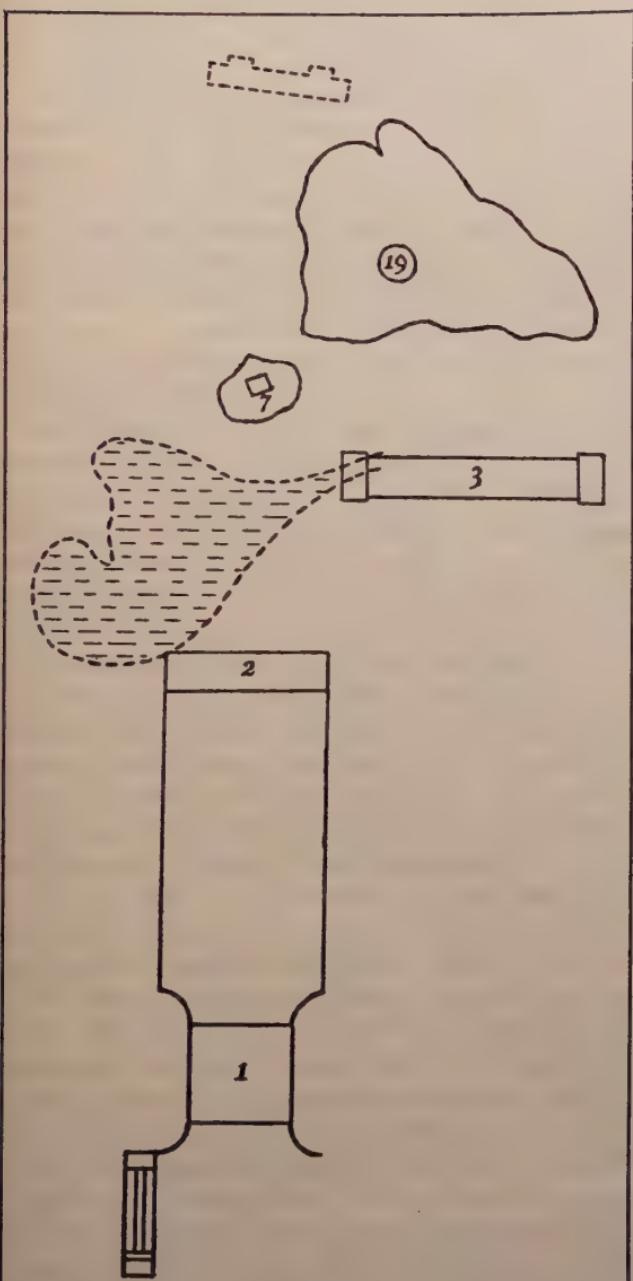


FIG. I

This diagram shows the positions of Nos. 1, 2, 3, 7 and 19 (with surrounding arboretum) on Richard's Plan of 1774 (in continuous outlines) in relation to the Petit Lac and (new) Orangerie (in dotted outlines) made later. The outline of the lake, enlarged to the scale of Richard's Plan, is taken from the Plan of 1898, of which there is an extract in the last Appendix of *An Adventure*. The full description of the latter is 'Plan du Grand et du Petit Parc de Versailles et des Trianons, levé et dessiné par M. Marcel Lambert, Architecte des Domaines de Versailles et des Trianons. Extrait de l'Ouvrage, Versailles et les deux Trianons, par MM. Marcel Lambert et Philippe Gille: 2 vols folio: Ed^r. L. Bernard, 17 Rue Hoche, Versailles.'

exactly opposite the middle of the north-west front of the house, at a distance of about 150 yards. The Plan shows a winding stream (not there in Louis XV's time) branching to form an island on which a kiosk is located on a *square* site. The position of that site, in relation to the now existing lake, is shown in Fig. 1. It will be seen that it falls some 15 to 20 yards north of the shore of the lake, in a place which is now apparently under a hill formed from the earth thrown up when the lake was made. To a person approaching from the gardener's gate, along the now existing middle path leading to the north-west corner of the lake, the kiosk, if visible, would have been seen in front and to the left, as the two observers said they saw their kiosk. Whether a kiosk ever existed at that spot I have been unable to discover.

The position assigned to their kiosk by the observers is shown on the sketch map at the end of A. There is no scale on the map, but, judging from the position of the Belvedere, the kiosk was *located* by them about 60 yards nearer to the gardener's house than the kiosk in 'Richard's Garden'. The observers obviously had great difficulty in placing it. The spot they fixed on is, so far as one can judge, in the line of sight of 'Richard's kiosk', as seen by anyone approaching from the direction of the gardener's house (as they did on 10 August 1901), but they seem to have 'pulled it back', to find suitable ground on which to locate it. There is, however, a strong indication that they had actually reached the edge of the lake near the Belvedere *before* they reached 'their kiosk', in which case it must have been located a little way ahead of them, on their left, very near the spot where Richard envisaged it. The reason for believing that they actually reached the north-west corner of the lake, and then turned to the right to go over the rustic bridge leaving the water on their left, is a remark recorded by Miss Jourdain in J1, but omitted from A : 'As we walked I found myself wondering whether anyone had ever stumbled over from the path into the water on our left.'¹ Mr Sturge-Whiting quotes this remark and pertinently asks 'What water? Miss Moberly noticed none'. He then argues at length that the two ladies must have reached the edge of the lake.² That conclusion is, no doubt, correct, but the inference that the building they saw was the Belvedere is not tenable. Apart from the fact that it does not answer to the description of the 'kiosk', it would have been behind Miss Jourdain when she nearly put her foot into the lake. The whole object of their sharp turn to the right at that juncture was to avoid going close up to the kiosk, partly because there was a very unpleasant-looking man sitting

¹ A, Second edition, p. 190.

² Sturge-Whiting, op. cit., pp. 43-5.

on it, and partly because another man urgently directed them to go to the right rather than to the left (see esp. A, p. 76). This incident is further dealt with below, and we must now go on to examine and compare the details of Richard's kiosk and those of the kiosk seen by the two observers.

On Le Rouge's Plate 23 is a design drawn by young Richard for site 7 on his father's plan. It is an enlargement of the little island shown on old Richard's plan with the kiosk in perspective standing on it. The structure is a slight one, consisting of four slender pillars, rising straight out of the ground, surmounted by a cupola-shaped roof rising to a point and finial in the centre. The edge of the roof is arched up (not turned up) between the four points of support. The preparation and engraving of Richard's plan and the plates relating to it must have been carried out as a matter of urgency during the summer of 1774, and the 'poverty' of this design, coupled with complete lack of detail in the bridges over the river, shown on the same plate, suggests that it may not have been intended as anything more than a 'token' design, pending the choice of something more elaborate later on.

Young Richard's design certainly answers to the description 'a light garden kiosk', and, although it has four pillars at the four corners of a square, as shown at site 7 on the plan, the shape of the roof makes it look 'round'. But it lacks a low wall, a balustrade, and steps, and is a much less elaborate kiosk than that described by the two observers. An attempt will be made later in this paper to throw further light on the design of the kiosk as described, and we can only note here that the *location* of the kiosk, after errors have been corrected, is surprisingly near the spot where the two observers said they saw one, before they turned to the right to go over a rustic bridge. They do not describe the detail of the bridge as they saw it, and in young Richard's design there is no detail of the bridge, which is a 'token' only.

2. *The barrier.* As the two ladies approached the House by a curving path, they saw on their left a (narrow) meadow of long grass, very much overshadowed by the trees growing in it (A, p. 47). On their right was a barrier entirely blocking the view. 'Originally, we could not see the steps [of the north-west front] whilst on the path, but after we had passed the barrier on our right hand we found them at once without going round any wall' (A, p. 92). No one has yet satisfactorily accounted for the barrier referred to above. It is very odd, therefore, to find in 'Richard's Garden' a fence, wall, or hedge, backed by a thick shrubbery, running from the small orangerie (No. 3) along the side of the formal garden outside the north-west front of the house.

One may be sure the 'barrier' was intended to be opaque, as there is such a marked difference between the formal garden inside and the park land dotted with trees and clumps outside. This fence or hedge would have blocked the view of the terrace on the north-west front of the house until it (the barrier) had been 'passed'. (An attempt to explain what this means will follow later.)

The picture of the *Jeu de Bague* created grave difficulty on the 1789 basis of dating, for it shows that structure in full view of anyone approaching the north-west front from the north. The two ladies were hard put to it to 'find' a mask of trees in the map of 1783 at the end of A, to account for the fact that they did not notice it.¹ But the map does not show whether there were *trees* around the *Jeu*. In the water-colour picture, which must have been painted in 1782 or later because the gallery is there, trees are shown standing in a semi-circle between the *Jeu* and the *Gallery*. The trees have tall bare stems and break out into foliage at roof level. They look as if they had been there some years. M. Rey mentions in his article (p. 122) that the accounts of Antoine Richard show that he planted mountain ashes (*sorbiers*) around the *Jeu*. The date is not mentioned, but it is hard to envisage an effective barrier of trees having been put all round the *Jeu* after 1782 and before 1789, in such a way as to hide it altogether. In 'Richard's Garden' there is, of course, no *Jeu*, as it was not built till 1776, and there is a 'barrier'.

3. *The 'second house'.* The Richard plan does not show much detail of the terraces and steps on the north-west and south-west fronts of the house, but it shows clearly a roofed building joined by a wall to the south-west corner of the house, with a narrow pathway, continuing at one level, along the south-west side of the building until some steps are reached just short of the door into the *Chapel*. In the absence of a contemporary picture of the end of the building, it is not possible to judge whether there was a door in it opening on to the terrace, but clearly there was a flat stretch of terrace between the end of the building and the south-west front of the House, providing a suitable 'stage' for the incident of the so-called 'Chapel man', who approached the two ladies 'on the level'.

4. *The wood with winding paths.* On 2 January 1902 Miss Jourdain paid her second visit to Versailles, and had a curious experience in a wood, described on pp. 60-1 and 101-2 of A. It was somewhere quite near the present *orangerie*. It had very tall trees, and well-kept paths wound between walls of under-

¹ A, p. 93.

growth that presumably consisted of evergreens, as Miss Jourdain could not see through them (A, p. 101). She also describes them as 'a maze of paths' (p. 60). Subsequent attempts by Miss Jourdain, Miss Moberly, and several others to find this wood failed (A, p. 101). Mr Sturge-Whiting found some young trees choked with brambles, when he went over the ground in 1937, and included a photograph of them as a frontispiece.¹ The claim that he had found the wood with very tall trees and winding well-kept paths is hardly substantiated. In 'Richard's Garden', however, we find just such a 'wood' or arboretum, surrounding a tulip-tree (No. 19), in the area where Miss Jourdain got into such difficulty. The winding paths are clearly seen on the plan.

At this stage the reader may feel that he has been led to the brink of sanity, and that it is time to pause and to review the situation. Let us ask ourselves some searching questions. First, were the two observers hallucinated at all? The reviewer of 1911 clearly thought not (*Proc. S.P.R.*, XXV, 360). J. R. Sturge-Whiting thought not. W. H. Salter says (*Journal S.P.R.*, XXXV, 186), 'I have no desire to attempt to prove that Miss Moberly and Miss Jourdain had no remarkable experience on 10 August 1901. They thought at the time they had had one, and when intelligent people in good health think that, they probably have. There are, however, many different kinds and gradations of experience, from vague to precise, from purely subjective to veridical.' G. N. M. Tyrrell and W. H. W. Sabine thought they were hallucinated (see p. 119 above).

It seems to me that in the narratives of the two observers there are unmistakable indications that on 10 August 1901 they were both deeply hallucinated as to sight and hearing, practically all the way from the gardener's house, where they entered the garden, until they entered the House. Apart from the fact that they saw objects which they could not afterwards find or satisfactorily identify, they described some in terms that are revealing to the student of parapsychology. I am assured by one who knew both the observers well that they did not suffer from any noticeable degree of short or defective sight. They could be relied upon to describe with reasonable accuracy anything they had seen in the course of a walk through a garden. When, therefore, Miss Jourdain says (A, p. 54, note), 'I remember that both [the woman and child] seemed to pause for an instant, as in a tableau vivant . . .', and Miss Moberly says (A, pp. 45-6), 'Everything suddenly looked unnatural, therefore unpleasant;

¹ Op. cit., pp. 61-3.

even the trees behind the building [the kiosk] seemed to have become flat and lifeless, *like a wood worked in tapestry*' [Miss Moberly's italics], we can recognize at once a picture-like quality which often characterizes hallucinations. But the most compelling evidence that the observers were hallucinated lies in what they did *not* see. During the whole of that walk, lasting perhaps half an hour, only eight persons were seen, of whom three were only seen by one observer. 'The chief features of our experience on that pleasant afternoon were the impressions of exceptional loneliness, and the extreme silence and stillness of the place' (A, p. 39). This was on 10 August, when the number of visitors to the garden must have been large. The day was cloudy, and there was some wind (A, pp. 44 and 49-50), but one gathers that the conditions were pleasant and not likely to have kept people away. When the two visitors returned on a July afternoon in 1904, they were astonished at the difference. 'In every corner we came across noisy merry people walking or sitting in the shade' (A, p. 66). In addition to the general considerations above, pointing to genuine hallucination, there are some very peculiar detailed effects, to be described later, which, if they are believed, can only be explained on the hypothesis that one or both were hallucinated.

I think, too, we must admit that Miss Jourdain was hallucinated on 2 January 1902, in the 'wood' near the orangerie (A, p. 60), and again on 12 September 1908, near the 'logement' (A, pp. 107-8). In the Authors' Preface (A, p. 41) referring to the first visit, they say, 'yet we were not asleep, nor in a trance, nor even greatly surprised—everything was too natural.' But I suspect this was drafted by Miss Moberly, who seems to have been less quickly and less deeply affected than Miss Jourdain. When the latter was describing her own feelings, soon after she had entered the garden on 10 August 1901, she wrote, 'I began to feel as if I were walking in my sleep ; the heavy dreaminess was oppressive.' (A, p. 54.)

Secondly, assuming that the two were hallucinated, what was the 'mechanism' of the process? I think the best way to picture it is to imagine that in front of the eyes of the subject a pair of spectacles is placed. The lenses are cut away at the bottom, so that the subject can look down his nose and see in the ordinary way his feet and enough of the immediate surroundings for him to walk about without straying or running into obstacles. But when he raises his eyes and looks through the lenses, he sees not the ordinary field of vision but a 'spectral' field, the objects in which are subject to what one might call 'the laws of dreams'.

While the subject is 'looking through the lenses', a 'part' of him is watching his steps all the time, and keeping an eye on his safety (as in the case of sleep-walkers).¹ Let us call this part the Monitor. If the subject hesitates through indecision, or is running into danger, the Monitor can send a signal to the dreaming self which is for the time being conscious. The signal can take the form of a 'hunch' to go on, to stop, or to turn aside, as the case may be, or it may be externalized into the spectral field of vision as a symbolic figure, giving directions or the like in a more or less dramatic manner.

This simile of spectacles is of course too mechanical to be accurate. The 'lenses' seem to form themselves and to melt away quite rapidly. While they are forming at the onset of hallucination, and melting away at the end of the period, curious optical effects are liable to be noticed by the subject.² He may see objects in the normal and spectral fields of vision at the same time. At the onset white objects in the spectral field show up first, and white objects in the normal field are the first to show signs of being suffused by the spectral field.

3. A REINTERPRETATION OF THE EXPERIENCES

In the light of this information, we can now follow the two ladies on the assumption that during their walk through the garden on 10 August 1901 they were seeing 'Richard's Garden'. Miss Jourdain was in trance, i.e. 'in Richard's Garden', so to speak, before Miss Moberly, and saw two 'officials' before she did, as well as a woman and child at a cottage, not seen by Miss Moberly at all. These figures are all described in terms that suggest that they were spectral. The two visitors seem to have been uncertain which way to go, and the two 'officials', who behaved like automata, may have been no more than the externalization of a signal from the Monitor to 'go straight ahead'. The two visitors then walked on. 'At last' (Miss J., A, p. 54) they encountered a path crossing theirs, and saw ahead the 'kiosk'. By this time they were getting near the Petit Lac. If they went past the Belvedere, they did not (in their condition) see it or the lake, for neither were features of 'Richard's Garden'. They saw a man at the kiosk with a repulsive face, unpleasant enough to make both

¹ Cf. also E. Swedenborg, *Heaven and Hell* (Everyman edition), p. 225, para. 441, where he describes a walk in trance lasting some hours during which he did not mistake the way.

² See in this connexion Note 1 on p. 667 of *Human Personality*, Vol. I, being a Note by E. Gurney cited by F. W. H. Myers; also G. N. M. Tyrrell, *Apparitions* (S.P.R. edition, 1943), pp. 65-6.

of them shrink from going on and walking past him. If they had gone on, they might have walked into the lake. So the 'Monitor' sent out an urgent signal which externalized itself as another man in a great hurry. The sound of his running footsteps behind them caused them to turn their heads and checked them in time to obey his urgent instruction not to go 'that way', i.e. towards the kiosk, but to go 'this way', i.e. to the right, and over a rustic bridge. At that moment Miss Jourdain must have looked 'under her glasses' at her feet, and realized that she had very nearly walked into the lake: 'I found myself wondering whether anyone had ever stumbled over from the path into the water on our left' (A, Second edition, p. 190). But when she 'looked through her glasses' again, she (and Miss Moberly) saw the spectral scene including Richard's bridge which was presumably in the Chinese style. For that reason Miss Jourdain did not recognize the rustic bridge which she had doubtless actually crossed (called the Rocher bridge) when she saw it in 1905 (A, p. 85).

Fortunately the signal had taken effect just in time and the two walked on until they reached the end of the old orangerie (no longer existing, shown at 3 on Richard's plan and (site) at 16 on M. Rey's plan at pp. 10-11 of his book). As they walked with the fence ('barrier') on their right, they seem to have swung to the left and then to the right, approaching the house, at an angle, towards the north-east corner. At this stage Miss Moberly seems to have 'lost her lenses'—she saw from some way off (A, p. 90) a woman sitting on a camp stool with her back to the house, apparently sketching the view from below the terrace. For Miss Moberly the fence in Richard's Garden had for the time being ceased to be visible and to Miss Jourdain it probably seemed as if they had entered the enclosed garden by the gate about halfway along the 'barrier'. 'We walked side by side straight up to her, leaving her slightly on the left hand as we passed up the steps to the terrace . . .' (A, p. 90). As the steps are in the middle of the terrace (of the north-west front), and as they passed the sketcher on her left hand (A, p. 48), she *must* have been on the clear ground on the left hand side of the north-west front (as one looks at it from the garden), and not on the right hand side, as they afterwards supposed. There is no 'large spreading bush' below the north-east window—see A, p. 65, and photograph of the English Garden front. For an artist, it was a natural choice to paint the view which Marie Antoinette saw from her boudoir every time she looked through the glass panes of the door which opens on to the terrace at the north-east corner of the house. 'Que de fois derrière ces vitres,

ou bien la fenêtre ouverte, de sa chambre et de son boudoir, la reine de vingt ans a dû prendre plaisir à voir grandir son rêve'.¹ Miss Jourdain, who was presumably still in trance, did not see the sketcher, but obviously passed very close to her. 'I remember drawing my skirt away with a feeling as though someone were very near me, and I had to make room, and then wondering why I did it' (A, p. 55). Her Monitor was clearly on the alert.

As Miss Moberly walked up the steps from the English Garden on to the terrace, she began to pass into trance again. 'I was beginning to feel as though we were walking in a dream—the stillness and the oppressiveness were so unnatural'.² When Miss Moberly passed close to the sketcher on the grass below she noticed that her fichu was white (A, p. 89), but when she looked back at her from the terrace, she noticed that her fichu was pale green (A, pp. 48–9). This observation, coupled with the statement just referred to that Miss Moberly was passing into trance again, as she went up the steps, is very interesting. It can hardly have been invented, and no one seems to have offered a satisfactory explanation. The attempt made by Miss Moberly to explain the green colour by reference to a green silk bodice which was made or repaired for Marie Antoinette in 1789 begs the question of the identity of the 'lady', and implies that the sketcher was wearing a green bodice (A, p. 91). There is nothing in Miss Moberly's account on p. 48 to imply this. The statement 'there was a little line of green or gold near the edge of the handkerchief' implies that the line was *on* the handkerchief, near the edge, and showed that the edge of it was visible and that it was 'over, and not tucked into her bodice'. The statement about the changed colour of the fichu proves beyond reasonable doubt (if we accept the statements at all) that the sketcher was a real person, and that when Miss Moberly looked back at her, the green of grass or foliage in the spectral scene, which, of course included no sketcher, was beginning to show 'over' the white fichu. If Miss Moberly had watched her for a moment or two longer, she would doubtless have seen her fade out altogether. (Compare the fading out of women's voices during the experience of 12 September 1908, see A, p. 108.) I therefore agree with M. Rey (Article, p. 121) in feeling that the theory that the sketcher was Marie Antoinette cannot be sustained.

While the two observers were on the terrace overlooking the French Garden, they showed signs of intending to continue their walk where the terrace 'was prolonged at right angles in front of

¹ Rey, *Le Petit Trianon*, p. 30. ² A, p. 48.

what seemed a second house' apparently on the level.¹ The end of this building, overlooking the terrace, is clearly shown on Richard's plan. Had they gone on, they would almost certainly have got into difficulties, as the building is no longer there, and the Monitor intervened. A spectral 'footman' came out of a door opening from the 'second house' onto the terrace, banging it after him, and approached them 'on the level'. We are not told how the man guided them down into the French Garden, but we may be certain it was by the steps, as they exist today. We may also be certain that he guided them out by the existing means of exit, though it may have looked different and seemed nearer than it really was.

The condition of trance probably continued longer in Miss Jourdain's case than in Miss Moberly's. She did not pass out of it till she reached the front entrance to the Petit Trianon (A, p. 55). It is worth noting that the condition only lasted while the two 'subjects' were in the *garden* (and court), and disappeared once they were in the house, where they felt 'quite lively again' (A, p. 49). It must also be emphasized that at no point in the foregoing reconstruction of the walk is the reader invited to imagine the two observers crossing a non-existent ravine by a non-existent bridge, or walking through a solid brick wall. On the contrary, the Monitor seems to have done his work well, in keeping their feet on terra firma throughout, and in preventing them from running into persons and obstacles.

We must now turn to Miss Jourdain's experience of 2 January 1902 (A, pp. 59-62). Judging from what she tells us of her feelings on that occasion, one infers that she went into trance for a few moments on her way to the Hameau, deeply enough to see a spectral cart being filled with sticks by two labourers. (These figures were probably a counterpart of the two 'officials' seen at the outset of the earlier experience on 10 August 1901, i.e. they were 'Monitor's men'.) The cart and men disappeared suddenly, and the presumption is that Miss Jourdain was 'herself' again all the time she was at the Hameau. In one or two places she felt an oppressive feeling, but there is no suggestion that she saw any spectral objects there.² She seems to have continued to see her normal environment until she had walked back from the Hameau

¹ A, pp. 49 and 55; also 94-7. It will be seen from Richard's plan that the 'second building' masked the Chapel door, and a person could not have been seen coming *out of the Chapel* by another person standing where the observers said they were. On pp. 94-7 the two observers have confused the 'second building' with the Chapel.

² The Hameau was started in 1783 and at the beginning of 1784 the houses in it were roofed. The gardens there were in the province of the

to a building in the 'old garden' which she recognized as the smaller orangerie.¹ Close to that she must have become rapidly entranced. While she could still see a belt of real trees to the left of the Hameau, she 'noticed a man, cloaked like those we had seen before, slip swiftly through the line of trees. The smoothness of his movement attracted my attention' (A, p. 60). Presumably this was a Monitor's man, moving in a different environment from the belt of trees—hence the bizarre optical effect, characteristic of incipient hallucination. In an other moment or two Miss Jourdain was 'deeply under', and seems to have found herself in 'Richard's garden' again, this time in the wood, with its winding paths, which can be clearly seen in Richard's plan. Of course she kept to real paths, and at one time she must have been passed by several ordinary visitors in a group. She 'felt' their presence, and heard the rustle of clothing and a spoken word or two, but did not see them, which suggests that she was in a dream world where real people are ghosts! Shortly after that she heard some music, coming apparently from the direction of the house. Music is an occasional accompaniment of hallucinatory experience, and from Miss Jourdain's description of it ('diminished in tone, as in a phonograph, unnaturally'—A, p. 61) I should say it was of that nature, and was not real music. I am not competent to judge the efforts subsequently made to 'identify' the music.

Miss Jourdain's description of her predicament at this juncture is amusing and extremely instructive. 'I looked at the map which I had with me, but whenever I settled which path to take I felt impelled to go by another' (A, p. 61). In other words, she could look 'under her spectacles' at the map in her hand, and choose a path to go by, but when she looked 'through her spectacles' she saw Richard's wood, and seemed to be following one of his quite different paths. At last she found herself back at the orangerie. If she could see and recognize that building she was presumably by that time coming out of trance. Assuming that she was in the curious state of vanishing hallucination, one cannot judge whether the very tall gardener (A, p. 61) was a real or a spectral figure. He certainly played the part of a 'Monitor's man' at an awkward juncture, and had 'gone' before she reached the Belvedere, which this time she saw as she passed it. From that point on there is no suggestion of anything unusual.

Lastly, there is Miss Jourdain's experience of 12 September 1908 (A, App. I, pp. 107-8). This was a very short trance, but the

Queen's Gardener, but as Antoine Richard died on 21 November 1784 his connexion with it was slight (Rey, *Le Petit Trianon*, p. 81).

¹ Rey, loc. cit., p. 11.

description of the scene, including the fading effects, stamps it as a spectral one. It took place as she was leaving the garden and she must have been very near the spot where her trance began on 10 August 1901. 'Once outside the lane, things became natural again' (A, p. 108).

The exposition I have given above of the various paranormal experiences related in A is open to criticism on two different counts, which must here be discussed. First, the reader may think that far too much reliance has been placed on detailed recollections of what the two observers saw and felt. Owing to the lapse of time before these were recorded, the recollections were liable to retrospective falsification. There is some force in this argument, but it does not explain why the two observers should have *erroneously* remembered details of psychological interest, the significance of which they clearly did not understand. After the experience of 10 August 1901 they seem to have concentrated their research on the historical aspects of the case. Why, for instance, should Miss Moberly have 'invented' the sequence 'the white fichu, the deepening trance, the pale green fichu', when it really furnished evidence *against* her theory that the lady seen was Marie Antoinette?

The second charge is much more serious, and more difficult to answer. It arises on the subject matter of the hallucinations. You have postulated, the critic will say, that most of the men seen were mere psychological dummies, symbolic figures, not representing real people. There is a strong presumption that the whole of the scenery in which they appeared was subconscious 'material' of the same character, and any attempt to link it with the year 1789 or 1774 or any other year is a waste of time. Any 'correspondences' found between the dream material on the one hand and the documentary evidence relating to this or that year on the other hand do not imply any paranormal cognition. All the 'English Gardens' in France in the eighteenth century, whether real or projected, were, in a sense, the offspring of a common conception, and they had a family likeness to one another. The 'waking dream' of these two English ladies was an unconscious improvisation, so to speak, on the theme of the English Garden at the Petit Trianon. It was much the same in 1901 as it was at the end of the eighteenth century, and an appreciable amount of correspondence was to be expected on grounds of 'family' likeness. The two ladies themselves thought they found in the events of 1789 patterns which were reflected in their visions. You think you have found a greater similarity of pattern in the 'Projet' of 1774.

The fact is that the treasury of historical material is so rich that anyone, with a little ingenuity, can 'find patterns' to support his pet theory, just as by delving in the rich treasury of Shakespeare's plays and poems five or six different and mutually exclusive theories of authorship by persons other than William Shakespeare can be 'supported' by 'evidence' that is ingenious, but fallacious.

The answer to this charge is that the *amount* of correspondence is what matters. In a given case 'family' likeness and chance can be regarded as responsible for a degree of similarity which can be called x . Readers will differ in their estimates of that amount. If the degree of similarity actually found exceeds x by a large margin, then some other factor must be operating. The reader will perhaps feel that in the case before us, the degree of similarity shown by the evidence so far produced does not exceed x . It is easy to exaggerate this degree of similarity by specious presentation of evidence. By adopting the symbolic method of interpreting some of the dream material, the critic may say, you have got rid of some difficult problems of identification, and have left for historical interpretation just those bits which seem to suit your case.

Still, I repeat, it is a question of degree. If the two observers had walked through the garden in an ordinary frame of mind, and if afterwards one of them, tired out, had sat on a seat and gone to sleep, dreaming that she was walking in an 'English Garden', where there were, vaguely located, a Chinese pavilion, a rustic bridge and cascade, and a wood with winding paths, I should have said that the evidence did not nearly reach the value x . But in the case before us two persons for about half an hour were collectively hallucinated, as regards nearly all the spectral objects seen. It looks as if a 'third party' influence was involved, confined to the gardener's domain, and 'interested' in things which, in relation to the garden, were to some extent never more than imaginary. It is not only the accurate locating of the kiosk, the barrier, and the wood, but also the choice of these as principal items in the dream which make it so difficult to account for them as the outcome of *mere* 'family likeness' and chance. The whole experience of the two ladies was clearly so puzzling to them that they 'lost their way', as it were, trying to understand it. It was not *their* imagination which invented the scenes. They must have felt that their interpretations of them were not the last word, for they say at the end of their Preface (A, p. 41), 'We record these things in order that they may be considered whenever the time shall come when a true explanation of our story may become possible.' At this stage a true, in the sense of a full, explanation is

not forthcoming. We are, perhaps, in the position of a man confronted by a large and difficult cross-word puzzle. Two long 'words' intersect across and down the middle. Tentatively, I have written in 'Antoine Richard' and 'Seventeen seventy four'. They may have to be rubbed out later. The clues themselves are not too clear—it is hard to know, in A, what are clues and what are not, quite apart from the difficulty of 'solving' them. If the two 'words' we have written in are correct, they may help with the discovery of other 'clues' and with their solution.

4. THE MAN AT THE KIOSK

So, in spite of his repulsive appearance, let us look closely at the man at the kiosk in case he may furnish a clue. Was he merely intended to play the part of a 'Monitor's man', and to frighten the two ladies away and thus to save them from falling into the lake? But that hypothesis would not explain why, of all kinds of repulsive or horrific looks, a face darkened and disfigured by smallpox should have been chosen. Whose imagination projected that particular horror on to the scene?

First, we must look back and see whether the reference to smallpox is 'early' or 'late'. W. H. Salter's Note conveniently summarizes the evidence on this point.¹ Miss Moberly, in M.1, describes the face as 'most repulsive'; in M.2 she says 'his complexion was very dark and rough'. Miss Jourdain in J.1 says 'his expression was very evil', and in J.2 adds that his face 'was marked by smallpox : his complexion was very dark'. It will be seen that it was Miss Jourdain who introduced the 'smallpox look', just as it was she who introduced the 'Chinese look' of the kiosk. She also noticed that his expression was 'unseeing', and adds 'though I did not feel he was looking at us, I felt a repugnance to going past him'. If the observers had afterwards exploited the 'smallpox look' in a convincing way, one would have suspected that it was a detail acquired later, and read back into the memory of the 'incident'. But all they could do with it was to attribute the face to a courtier, the Comte de Vaudreuil, 'a Creole, and marked by smallpox' (A, p. 75). M. Rey in his article (p. 121) dismisses that identification with a smile. The grounds are so weak that one feels the 'smallpox look', like the 'Chinese look', was an embarrassment and not a help. It was very difficult to 'make sense' of it. For this reason it sounds like a genuine recollection. Miss Moberly accepted as correct that the man's complexion was 'very dark and rough' (M.2, written, no doubt, after

¹ Loc. cit., p. 181.

discussion with Miss Jourdain), but may have hesitated for want of knowledge to attribute the condition to smallpox. (It is fairly certain that neither lady had ever seen the face of a man in an advanced state of smallpox.) The man at the kiosk was alive—he turned his head—but seemed not to be able to see.

If we look at this curious grouping of objects, we are faced with two different images which seem to have no connexion with one another, (1) a garden pavilion in the Chinese style, on a round base, with features resembling those of two of Sir William Chambers's designs, and (2) a man disfigured, darkened in colour, and rendered sightless by smallpox. It can be proved by documents that these two images were part of the experience of a man, living in 1774, who knew Antoine Richard well, and it can be inferred that they became, at first or second hand, part of Richard's experience as well. The two images are also curiously relevant to the year 1774 and to the garden of the Petit Trianon. The man in question was the Duc (or Prince) de Croy (1718-84), who assiduously attended the court of Louis XV, and, being a keen naturalist, gladly spent an hour or two, when he could, discussing trees and shrubs with Antoine Richard.¹ The Duc left voluminous memoirs in the form of a *Journal*,² published in four volumes, beginning in 1906, and these suggest that the scene at the kiosk, as described by the two ladies, may have remarkable significance in relation to the events of the year 1774.

As regards the design of the kiosk, there is in *Le Rouge* a plate³ which shows, on each side of a vertical line bisecting the plate, two half elevations. On the right is a design, headed 'Pavillon Chinois, nommé Tings'. It is an exact copy of Chambers's design on his Plate VI, Fig. 2 (see right-hand design of Fig. 2 of the present article). The reference to 'Tings' also confirms that it was got from Chambers's book *Designs of Chinese Buildings* etc. in which he mentions (p. 6) that his design was one of four different sorts of Tings (a Chinese word), and was copied from an original in a garden at Canton. On the left of the plate is a design showing the same main features as those on the right, but drawn for an octagonal base, and differing only in ornamental details. It is headed 'Projet d'un KIOSK pour le parc de l'Hermitage dessiné par M^r le Prince de Croy'. The plate is not dated, but it was almost certainly made in 1774. Very few of the plates in *Le Rouge* bear dates. This one can hardly be later than 1774, as it comes in Cahier 4—i.e. earlier than Antoine Richard's *Projet*, dated 1774,

¹ See *Journal*, cited in Note 2 below, Vol. I, p. 408.

² *Journal Inédit du Duc de Croy*, with Introduction, Notes and Index, (Paris, Flammarion, 1906).

³ Cahier 4, No. 15.

which comes in Cahier 6. It is also unlikely that de Croy made use of Chambers's design before Chambers's visit to Paris in 1774. That visit took place in the summer of that year,¹ and, although de Croy does not mention having met him, there are some observations in the Journal, written at the end of August 1774, about the Chinese style in gardens,² which reflect the views of Chambers. De Croy was a 'decided enemy of England'³—he fought with distinction at Fontenoy—and he was perhaps unwilling to acknowledge indebtedness to an Englishman.

We know, then, that de Croy used one of Chambers's designs for a kiosk in his garden, and he may well have sent a copy of his 'Projet' to Antoine Richard. Alternatively, it is possible that Richard was influenced by Chambers's design for the Kew Gardens pavilion mentioned above. There is no doubt that Richard's ambition was to make his Botanical Garden into a French counterpart of Kew Gardens,⁴ and it is fairly certain that he had seen pictures of the Chinese features at Kew. In this field the French were by their own admission copiers of the English,⁵ so it is not idle speculation to look to English sources. Further, the possibility that a kiosk, perhaps of a more elaborate description than that contemplated by young Richard, was actually constructed, is suggested by two considerations:

(1) On the old map brought up to date by Marcel Lambert (see last map at end of A) 'Le Kiosque' is shown just below 'Pavillon de Musique' (i.e. the Belvédère).⁶ It is not clear what exact point on the plan is indicated, but there appears to be a small narrow promontory projecting into the lake, with a dot on it, intended to represent the kiosk. The promontory is not shown in Mique's plan of 1783.

(2) When de Croy visited the garden on 8 June 1782 he found 'l'Anglais, et la Chine partout'.⁷ This remark suggests that a considerable number of details in the Chinese style still remained all over the garden. Is it possible that they remained there till old Richard died in 1784, and were then (apart from the Jeu de Bague) ruthlessly swept away? It is certain that in 1785 there was acute tension between Mique the architect and young Richard, who had succeeded his father, as we learn from a very interesting

¹ T. Hardwick, *Memoir of the Life of Sir William Chambers* (London, 1825), p. 9. I have tried without success to discover the dates of Chambers's arrival in and departure from Paris.

² *Journal*, Vol. 3, pp. 145–6.

³ *Ibid.*, Vol. 1, p. xxix.

⁴ *Ibid.*, Vol. 4, p. 218.

⁵ *Ibid.*, Vol. 3, p. 145.

⁶ See A, p. 74; it is said the map was an old one of 1705, 'marked up' to about 1898.

⁷ de Croy, Vol. 4, p. 263.

and entertaining account of a visit by Thomas Blaikie, the Scotch Gardener, to the Petit Trianon in May of that year.¹ He describes Claude Richard as 'frequently at variance with one M. Micque who is the head of all, as those architects has the pretention of knowing everything, and what they say is looked upon as law and the gardener must submit to their ignorance or be exposed to lose his place'. Whether some kiosk which Antoine Richard had in mind in 1774 for Site No. 7 ever materialized or not, it was doubtless the focus of strong feeling.

5. MISS MOBERLY'S SKETCH OF THE KIOSK

In the Bodleian Library there is a collection of 'Maps and Pictures (gradually collected by E.F.J. and C.A.E.M.) of the Petit Trianon after 1901 and 1902. Arranged by C.A.E.M. March, 1928' (Reference MS. Engl. Misc. C. 257). On page 19 there is a pencil sketch of the kiosk, on a piece of paper, about $5\frac{1}{2}$ inches by $3\frac{1}{2}$ inches, affixed to the page. Bodley's Librarian has kindly given permission for the sketch to be reproduced here (Fig. 4). Below it are the descriptive matter and following notes by Miss Moberly.

Returning for the first time to the Petit Trianon in 1904 and finding that 'the Kiosk' was no longer there, I made this hurried drawing of it from recollection—as seen in 1901. Authorities show us that there was a 'naissance de la rivière' in that position above the lake. Mique states that he placed, in 1780, 'une petite d'architecture' above the 1st grotto. Desjardins says that the Queen rejected a 'ruine' copied from one at Baalbec. Note p. 90, 'D'après la 44e-planche des ruines de Baalbec, elle représente un édifice circulaire d'un dessin très élégant. Voir. Les Ruines de Baalbec par Robert Wood, Londres, 1737² in folio, Fig. . . .'.³

We looked up this 'planche' in the Bodleian Library, and finding that it had resemblances to our kiosk (both were round, had low walls, pillars, and a roof with a *slightly Chinese effect*) thought that it might have been copied with modifications. A 'ruine' stands for a copy of an older building. In an old map produced in 1909, the name 'Le Kiosque' represents something exactly in this part of the garden, so that we instinctively gave it the correct name.

Considering that the sketch was made in 1904, some three years after the event, it is perhaps captious to complain that it does not show all the details mentioned in the narrative descriptions, written three or four months after. It must, however, be noted that Miss Moberly, who in her earliest written version (A, Second edition, p. 185) referred to a 'balustrade', does not, in her sketch, show anything that clearly represents a balustrade. Nor are any

¹ T. Blaikie, *op. cit.* p. 192.

² This date is clearly wrong. See note on p. 119 above.—G.W.L.

steps clearly indicated. The roof—so far as one can see it—looks like a low-pitched cone, with the edges very slightly flattened out. On the outside there seem to be ridges running from the apex to the eaves. The pillars, of which five are visible, were probably ten in number. They are slender, and seem to be enlarged towards their bases, which stand upon a floor forming the top of a low cylindrical basement. It is just possible that the low circular wall was ornamented with some sort of pattern that made it look like a balustrade.

Compare now the design in the sketch not with the 'Baalbek design', to which the two ladies tried to liken it, but with Chambers's design No. 1 at Fig. 2 (the left-hand design). Common to both is the circular plan—a rather unusual feature—a low conical roof with ridges on the outside, ten slender pillars enlarged towards their bases, and a low basement. Chambers's design shows a roof with its edges less abruptly flattened and a stepped basement, but it is unlikely that a design prepared for the Petit Trianon would have been an exact copy of a Chambers's design in all details. (We know that the Duc de Croy modified somewhat the details of Chambers's design No. 2 of Fig. 2 for his own kiosk.) In short, whether we rely on the written descriptions of the kiosk or on Miss Moberly's sketch, we seem to be driven to the conclusion that the structure seen by the two ladies 'belonged' not to the beginning of the twentieth century but to the last half of the eighteenth century. As to the likelihood of there being Chambers's influence in a design prepared for the Petit Trianon Garden, I have already drawn attention to the parallel between the Royal Botanical Garden at Kew and the French Royal Botanical Garden. It can hardly be mere coincidence that Mique's Temple de l'Amour (apart from the Cupid inside, which is a separate work of art) is practically a replica of Chambers's Temple of the Winds (or of Aeolus) at Kew. The projected 'ruine', based upon the Baalbek design, is practically a replica of Chambers's Temple of the Sun (or Apollo) at Kew, and Richard's design for a Chinese aviary, to stand on Site 12 in his plan (Fig. 3), was clearly inspired by Chambers's Chinese aviary at Kew. One is tempted to speculate whether, during the summer of 1774, after the death of Louis XV, and after Richard's plan had gone to the engravers, there was discussion with Sir William Chambers himself, while he was in Paris, about a kiosk for the Queen's projected English Garden. And did the project ever materialize?

The other image was that of a man whose face was dark and marked by smallpox. Here is a plain recital of facts taken from de

Croy's *Journal*. On 10 May 1774, Louis XV, who built the Petit Trianon, died of smallpox at Versailles. He had contracted the disease at the Petit Trianon near the end of April, and was moved to Versailles before it was diagnosed. The Duc was in attendance and saw the King almost daily up to the time of his death. On 6 May the King's face, which till then had been red and swollen,¹ began to turn dark.² Owing to the scales on his eyes, he could not see clearly.³ On 8 May his condition became very grave and the doctors despaired of his life.⁴ The Duc, as he tells us later on, went that day to the Petit Trianon and told his friends Antoine and Claude Richard the turn of events, no doubt describing to them the appearance of the King. They exchanged condolences with heavy hearts, and the Duc said good-bye—he did not see them again for about six years.⁵ De Croy then returned to Versailles, and awaited the end. On 9 May the symptoms became worse, and by midday there were few signs of life.⁶ Late that evening de Croy saw the King alive for the last time. His face was 'a mask, as of bronze . . . like a moor's or negro's head, copper-coloured and bloated'.⁷ After death, for fear of infection, the body was put into quicklime in a lead shell, and was buried without ceremony.⁸ The sight of the dying King made a great impression on de Croy, who twice refers to it as 'frightful' (*affreux*).⁹

Considering that the *Journal* from which I quote was not published till 1906–7, it is very curious indeed that two English ladies should have seen in 1901 that very dark and rough face on the man at the kiosk. It is still more curious when one reads the following paragraph from de Croy's *Journal*: 'Many rumours were started about the manner in which he [the King] had contracted that disease, but the fact was that some children in the neighbourhood had it. One little girl, two years old, died of it in a garret at the end of the Garden of the Trianon ; the body was carried away by night, wrapped in a cloth, and it would seem certain that that caused the poison to be spread about in the garden, where he often went.'¹⁰ When de Croy saw Richard years later, in 1782, they recalled this account of the origin of the disease,¹¹ so it was common knowledge between them. The circumstances in which

¹ de Croy, Vol. 3, pp. 89, 92 and 93.

² *Ibid.*, p. 99 'Le visage paraissait plus noir. . . .'

³ *Ibid.*, p. 103. ⁴ *Ibid.*, p. 101.

⁵ de Croy, Vol. 4, pp. 217 and 263. ⁶ *Ibid.*, Vol. 3, p. 102.

⁷ *Ibid.*, p. 104. The smallpox was of the 'confluent' variety, which causes shocking disfigurement.

⁸ *Ibid.*, p. 111. ⁹ *Ibid.*, pp. 103, 104.

¹⁰ *Ibid.*, p. 107. ¹¹ *Ibid.*, Vol. 4, p. 263.

Louis XV died are, of course, a matter of widely-known history. Carlyle's *French Revolution*, which Miss Moberly had read in the schoolroom,¹ opens with 'The Death of Louis XV'. The author mentions smallpox as the cause of death but there is no reference to the blackening of the face.² Further, in the context of the experience, the clue to that death, if it is a clue, is 'tied up' to the kiosk, and the connexion between the kiosk, as they describe it, and the death of Louis XV could not have been discovered and used, even subconsciously, by the two observers, using ordinary sources of information, unless they had studied both Le Rouge's engravings and the *Journal* of the Duc de Croy (still in MS in 1901). One may be certain they had not seen either work before 10 August 1901.³

For the Duc and Antoine Richard the death of Louis XV was a very severe blow. For the old gardener it meant the loss of a sympathetic master and the beginning of great upheavals in his garden. Almost immediately the Petit Trianon was given to the young Queen Marie Antoinette, who wanted the botanical garden turned into a 'jardin anglais', of a kind which was then the fashion. Plans were invited, and Richard prepared his 'Projet'. It was rejected, as I have said, in favour of one offered by an amateur, M. de Caraman. Richard's precious botanic garden and its hot-houses were uprooted, and placed elsewhere; a room in the house which Louis XV had used as a botanical library was turned into a bedroom for Marie Antoinette; and a new and masterful architect, Richard Mique, was put over the gardeners. During the last ten years of his life Antoine Richard must have suffered from an acute sense of frustration. His feelings may be judged from the entry in Blaikie's diary for Saturday, 5 October 1777 '... went to Trianon—here is great alterations going on—here we met with Mons. Richard who showed us very civilly the gardens ... this [place] belongs to the Queen who is not fond of plants—they are turning it all into a sort of English garden, what a pity such a valuable collection should be destroyed! This seems much to affect Old Mr Richard'.⁴ When de Croy returned to the Petit Trianon in April 1780, he also found the place so changed that he thought he was mad or dreaming.⁵ He admired some of the new features, notably the 'Palais de l'Amour', as he calls it, but was shocked at the mixing up of the 'ton grec' with the 'ton chinois'. He does not mention the *Jeu de Bague*, but doubtless

¹ A, p. 43, note 2.

² Chapman & Hall Edition, p. 5.

³ See A, p. 43, as regards Miss Moberly's slight knowledge of French history, and p. 52 as regards Miss Jourdain's ignorance.

⁴ T. Blaikie, op. cit., p. 136.

⁵ *Journal*, Vol. 4, p. 217.

had it in mind when he made that remark. He found old Richard busy planting 'une allée tournante', with trees of every species and variety on each side of the path.¹ It looks as if, after all, old Richard's plan for a wood with a winding path materialized also.

Whatever documentary sources the two ladies may have 'tapped', even subconsciously, there seems to be a sort of pattern in their visions which would not arise from a simple mixture, in the memory of either observer, of the ingredients contained in (say) Le Rouge's plates *plus* the first four chapters of Carlyle's *French Revolution*. The objects seen in the garden are a variation of those in the plans. The kiosk is not the same. Nor was it seen to be on an island or promontory, perhaps because the river in the spectral scene had become so small that it was lost to sight above and below the rustic bridge. These differences may be significant, or they may be just those odd variations which one might expect in the process whereby an idea is externalized in hallucinatory form. Yet behind them all there is that ingenious selection of material which seems to imply somewhere in the background a 'selector' who has very cleverly chosen separate pictorial clues which all point to the year 1774. On the face of it there is an anachronism, in that the death of the King on 10 May preceded by a few weeks any design of a kiosk for Marie Antoinette's English Garden. As a matter of history Louis XV could never have sat on the steps of any kiosk constructed in pursuance of Richard's plan. It may be that there was an earlier kiosk there in Louis XV's time. But are we dealing here with strict history? Are not the clues (if they are clues at all) symbols, not necessarily arranged *inter se* with strict regard to chronology?

W. H. W. Sabine, in his article 'Is there a case for Retrocognition?' suggests that in cases of this kind the subjects are really precognizing the results of future research.² The discussion is conducted on the footing that in these cases the problem is confined to 'cognition', 'knowledge', 'awareness', and 'information'. Is there, in relation to these, a forward or backward displacement in time? I find it difficult to explain, on that theory, a case like the present one in which the material 'cognized' by the subject in the first instance is not *mere* information, or *mere* awareness of an apparent matter of fact, but is a complicated pattern of ideas and information, the significance of which may or may not be verified by subsequent research. Such cases do not seem to be explicable either by simple retrocognition or by simple precognition. At the time of the experience the 'selector' seems to know the

¹ *Journal*, Vol. 4, p. 218.

² *Journal of the American Society for Psychical Research*, XLIV, 43.

past and to dramatize it into the present, not as a mere repetition of the past but as a dream-like representation of it, using symbols which the subject in his normal state finds it hard to understand. I think this conception is much the same as that to which G. N. M. Tyrrell gave expression in his Myers Memorial Lecture *Apparitions* when he wrote :

Again, if the account of Miss Moberly's and Miss Jourdain's experience in the Gardens of Versailles be accepted, the case does not differ *in principle* from that of a recognised apparition performing some action which was habitual with the recognised person during his life-time. The difference lies in the scale of the environment. Granted that the knowledge was somewhere in existence of what the Gardens were like in 1789, and that somewhere there was an agent (I think this is necessary) to give dynamic force to the *idea* of the Gardens and make it operate on the percipients at the particular time when they were in them, their percipience of the Gardens as conceived in the idea is merely a matter of the accident of their being on the spot when they happened to be *relevant* to the idea as spectators, and that they happened to be sufficiently receptive to react to the idea and give it sensory expression. There is no need, on this view, to invoke retro-cognition.¹

Later on he writes, 'The case, if we accept the evidence, bears the marks of being telepathic rather than a subjective reflection of forgotten knowledge of their own.'²

The postulate that 'somewhere there was an agent' leaves open the question whether the agent was someone living at the time or someone (in ordinary parlance) dead. The 'agent' here would seem to fulfil the same role as my 'selector' mentioned above. I have not tried to identify the selector, as to do that would require a great deal more detailed knowledge than I possess about the mechanisms involved in producing hallucinations of the kind here in question.

SUMMARY AND CONCLUSIONS

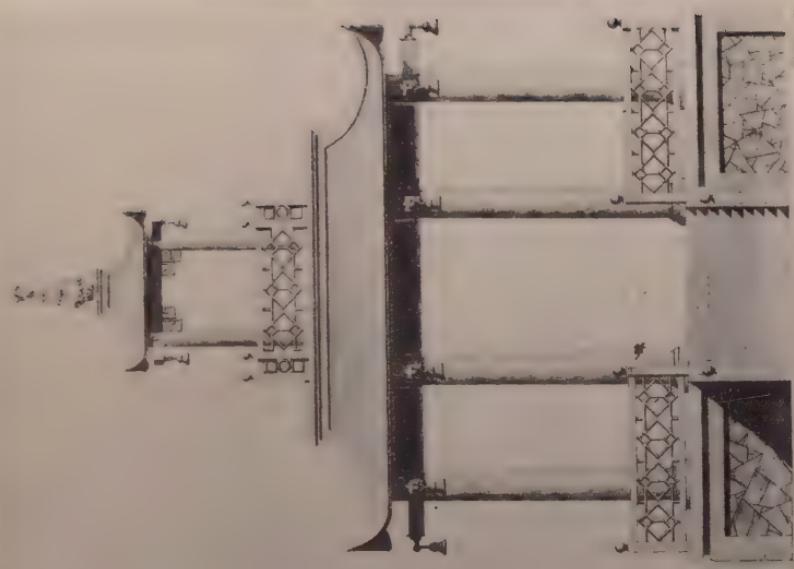
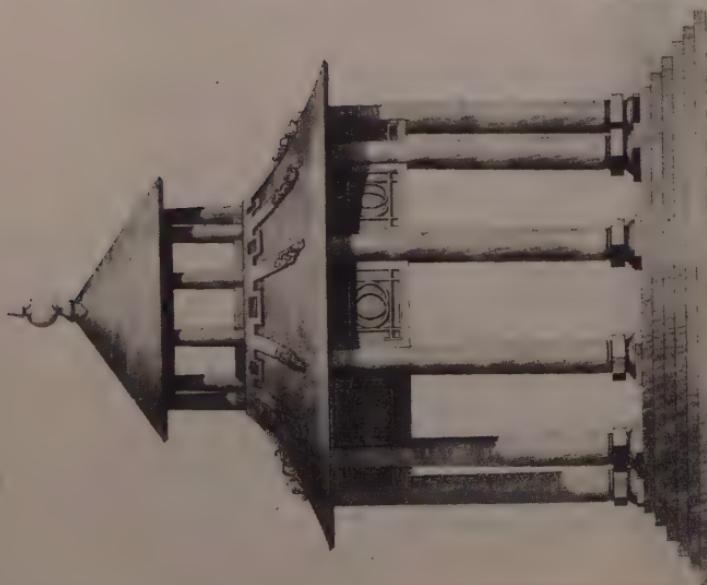
Up to this point the clues and evidence have been set out more or less in the order in which they presented themselves, with some awkward interruptions to discuss doubtful points in the evidence afforded by A. It is now time to draw the findings together. The main headings are :

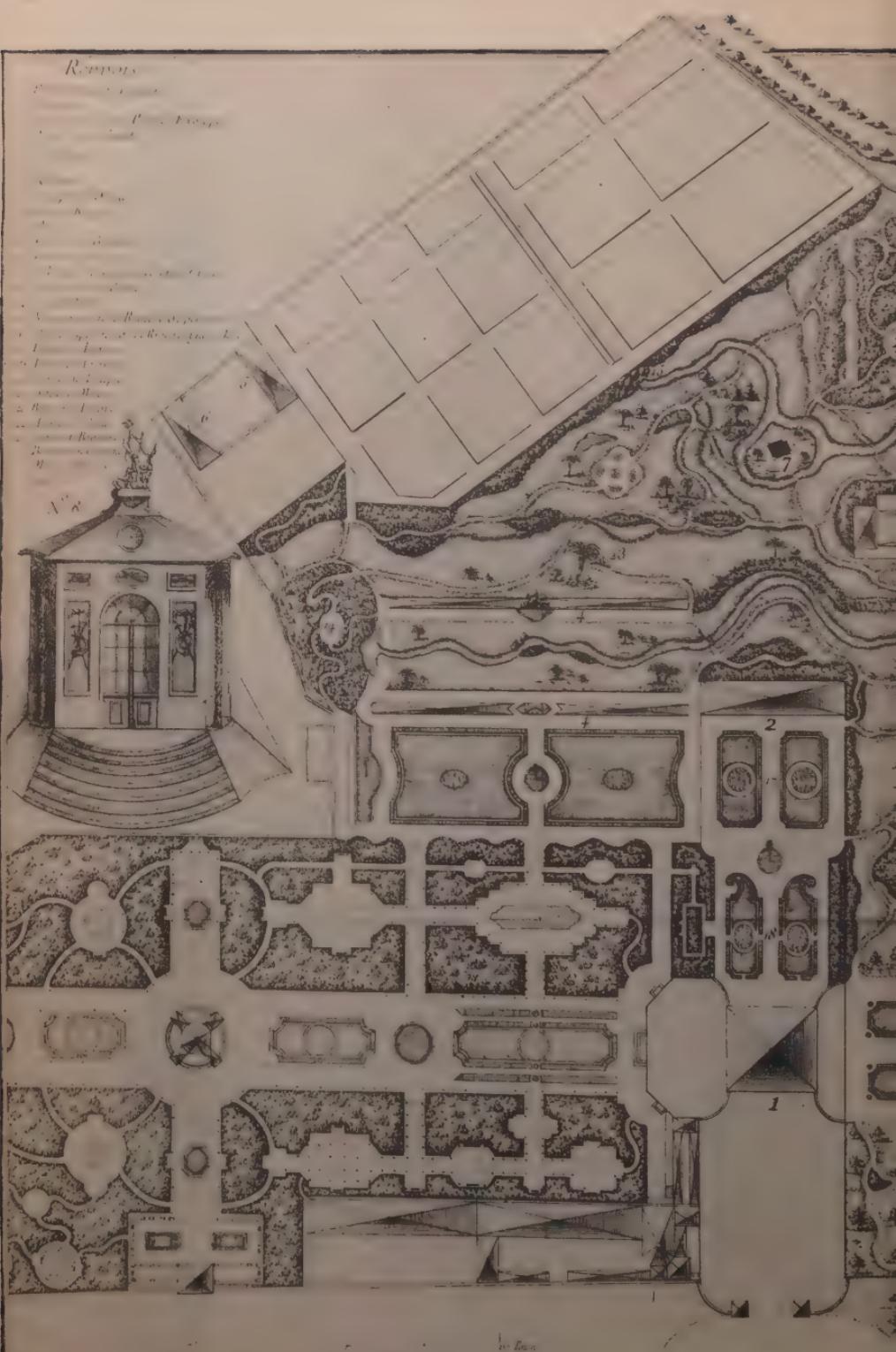
1. *Incidence of 'trances'.* All trances deep enough to result in visual hallucinations took place inside the area served by the gardener. In the case of Miss Jourdain, who visited the Petit

¹ *Apparitions* (S.P.R. edition, 1943), p. 69.

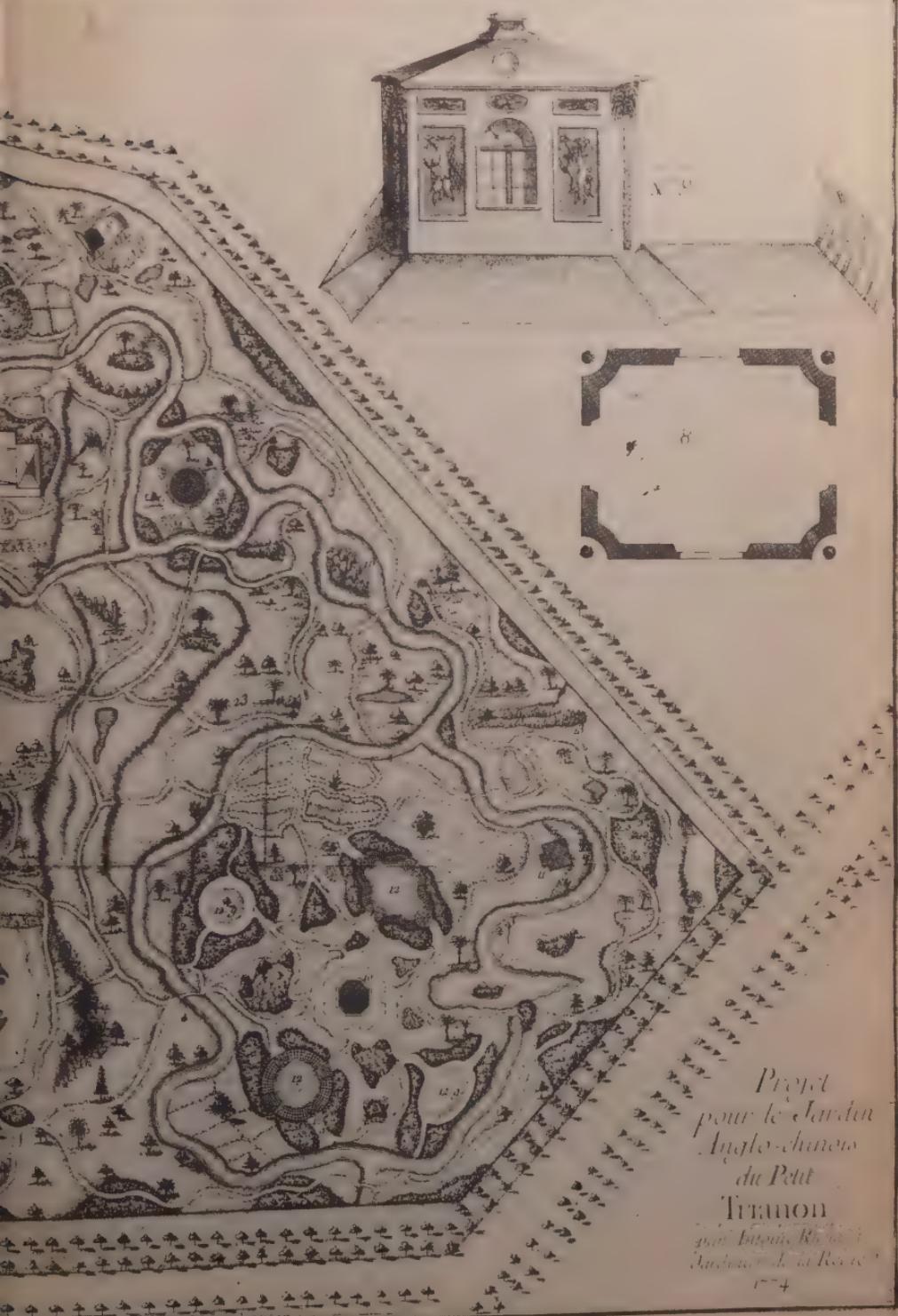
² *Ibid.*, p. 71.

FIG. 2
PLATE VI IN 'DESIGNS OF CHINESE BUILDINGS' BY SIR WILLIAM CHAMBERS
(LONDON, 1757)





PLAN, DATED 1774, BY ANTOINE RICHARD

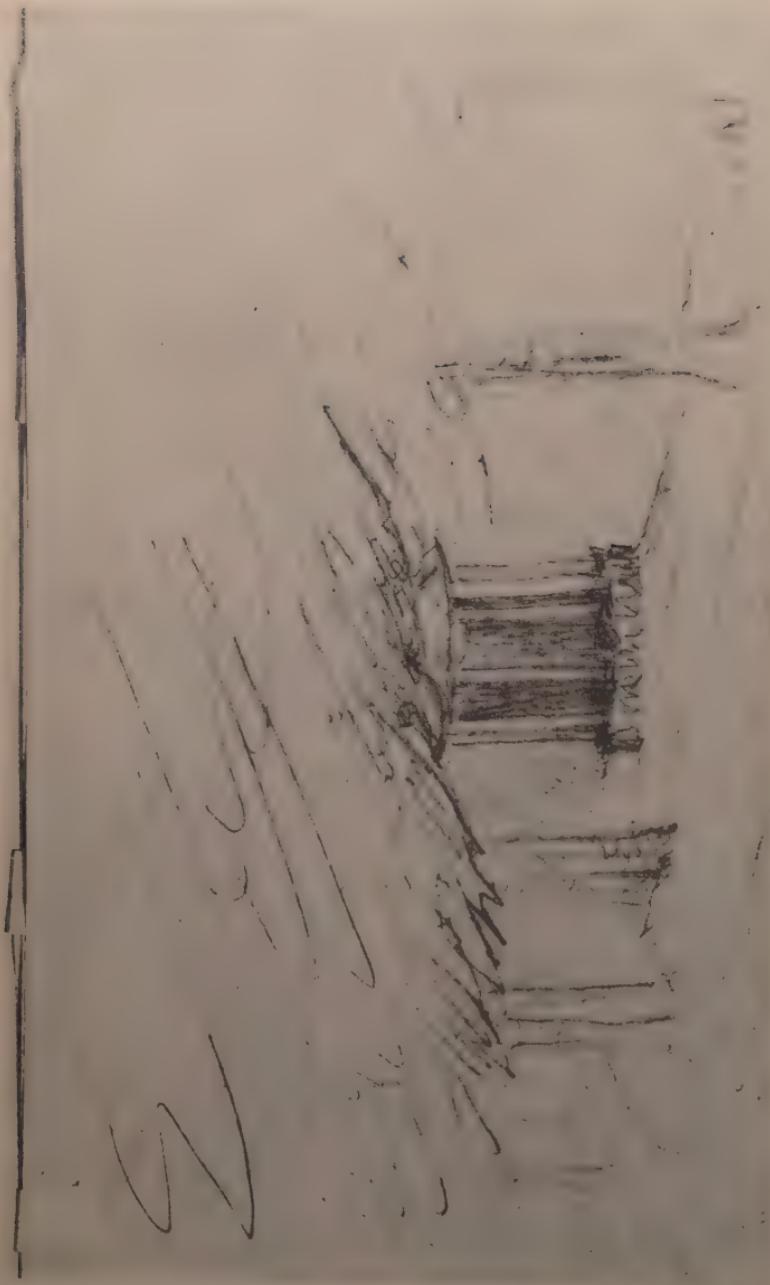


Project
pour le Jardin
Anglo-chinois
du Petit
Trianon

par J. J. R. et
J. C. de la Roche

1774

FIG. 4
SKETCH OF THE KIOSK, AS SEEN IN 1901, DRAWN FROM MEMORY BY
MISS C. A. E. MOBERLY IN 1904



Trianon more often than Miss Moberly, the 'density effect' seems to have been greatest in the neighbourhood of the gardener's house (10 August 1901 and 12 September 1908) and the north-west part of the English Garden (10 August 1901 and 2 January 1902), and less in the direction of the Hameau (2 January 1902).

2. *Positions of garden features.* Both observers saw three features, a kiosk, a 'barrier', and a building at right angles to the house, which appear in Antoine Richard's *Projet* of 1774 but were not there in 1901; and one feature, a bit of terrace which was level in 1774 (and 1783) but not in 1901. One observer (Miss Jourdain) saw a wood with winding paths, of which the same can be said. In all these cases, if allowance is made for a slight error in 'placing' the kiosk afterwards, the position of each feature seen is so close to the position of the corresponding feature in Richard's plan that there appears to be a 'dating correlation' signifying the year 1774. The force of this observation, in relation to the position of the kiosk, is not weakened by the fact that as seen and described by the observers it does not exactly tally with the design prepared for it by young Richard.

3. *Dramatic effects.* Apart from the woman and child at the entrance and the man at the kiosk, the spectral figures seen in the garden were six men, all of whom appeared on the scene, played their small parts, and disappeared again exactly when required to act as what I have called 'Monitor's men'. This theory that they were dream figures projected by the needs of the moment does not exclude the possibility that there was some significance in their outward appearance or dress. Some further research is being undertaken into this problem. Meanwhile, it is to be noted that the design of the kiosk, if it has been correctly traced to Sir William Chambers, has a dating significance which in France is also, probably, 1774, the year in which he visited Paris.

The most striking dramatic effect in the experience of 10 August 1901 was the face of the man at the kiosk. It would have been extraordinary if the complexes or suppressed memories of two unrelated individuals, with different mental histories, had projected into consciousness the same image at the same time and place, so the face does not lend itself easily to a psycho-analytic interpretation. On the other hand, there was 'in the place' a history of a very strange and moving kind, heavily charged into emotion, starting about 10 May 1774, the day on which King Louis XV died, giving the visions a significance which it would be hard to match for relevance from any other period in the history of the Petit Trianon.

Further, it should be noted that the interpretation given above involves no 'reminiscences' of the tragic events of 1789 and the following years. Both Antoine Richard and the Duc de Croy died in 1784, within a few months of one another, so they did not live to see those events.

If the reader should ask, 'Against what kind of a background could two ladies in 1901 possibly come in contact with images and "memories" which belong to the year 1774, and "see" a garden which, so far as we know, never completely existed outside the imagination of Antoine Richard?', I would advise him to read, a second time if he has already studied it, Professor H. H. Price's admirable lecture 'Survival and the Idea of "Another World"' (*Proceedings S.P.R.*, Vol. 50, Part 182, January 1953). He says (p. 18), 'All I want to maintain, then, is that there is nothing self-contradictory or logically absurd in the hypothesis that memories, desires and images can exist in the absence of a physical brain. The hypothesis may, of course, be false.'¹ If the hypothesis is false, I do not myself see how one can begin to explain experiences like those of Miss Moberly and Miss Jourdain.

I am indebted to my friend Sir Eric de Normann for drawing my attention to M. Rey's article. It acted as a stimulus, without which this paper would not have been written. I am also indebted to the Editor of the *Journal* for several references to earlier literature on the subject of *An Adventure*, and for constant help and encouragement. I have also to thank the Trustees of the British Museum for permission to publish Figs. 2 and 3 and Bodley's Librarian for allowing me to publish Fig. 4. I am also grateful to Mr W. G. Roll, President of the Oxford University Society for Psychical Research, for making an initial copy of Miss Moberly's sketch. Finally, I would emphasize that the foregoing paper is only a preliminary attempt to interpret *An Adventure* in the light of the events of 1774. Doubtless the exposition of the case would have been clearer had I quoted at greater length from the book at each stage, and supported the argument with more numerous illustrations. This would have involved a prohibitive addition to the length of this paper and to the cost of printing it, and I must crave the reader's indulgence. There is scope for a good deal of further research, especially into French archives, which might well throw more light on the mystery. I hope enough evidence has been adduced to make the task of further inquiry seem worth while.

¹ See also Professor Price's Presidential Address in *Proceedings S.P.R.*, Vol. 45, Part 160, December 1939.

HOME-TESTING ESP EXPERIMENTS
SPECIAL REPORT ON ONE SERIES OF TESTS
BY A. M. J. MITCHELL

1. Introduction

In 1951, the writer was invited by G. W. Fisk to take part in the S.P.R. Home-Testing Experiments (1) and was able to conduct tests on nine subjects under GESP conditions. It is the purpose of this report to describe a series of tests with one particular Agent and Percipient in which outstanding results were obtained. This is the case referred to by Dr D. J. West in his examination of displacement effects observed in the Home-Testing Experiments (2). Briefly, Messrs D.C. and M.B. acted as *A* and *P* respectively in nine sessions of 100 trials per session, using ESP cards under GESP conditions. The total score was significantly above chance, giving a *t* value of 5.80 ($P=6.6 \times 10^{-7}$, odds of 150 million to one against). The results are summarised in Table I and Fig. 1.

TABLE I
SUMMARY OF HITS PER SESSION FOR D.C./M.B. GESP TEST

Date	Location	Hits (Dev. from MCE)			Average secs./call	t value		
		-1	ON	+1		-1	ON	+1
12.6.51	Laboratory	+4.8	+15	+0.8	0.6	+1.22	+3.75	+0.20
13.6.51	"	-3.2	+16	-4.2	13.2	-0.82	+4.00	-1.07
5.7.51		+5.8	+56	-3.2	8.1	+1.48	+14.00	-0.82
6.7.51	Hotel	+6.8	+23	+2.8	0.6	+1.73	+5.75	+0.71
13.7.51	"	-0.2	-6	-4.2	6.8	-0.05	-1.50	-1.07
19.7.51	Laboratory	-6.2	-9	+2.8	5.9	-1.58	-2.25	+0.71
17.8.51	"	+5.8	-10	-0.2	0.6	+1.48	-2.50	-0.05
20.9.51	"	+17.8	+3	+22.8	7.2	+4.53	+0.75	+5.81
16.1.52	"	+4.8	0	+1.8	4.8	+1.22	0	+0.46
Total		+26.2	+88	+19.2		+2.33	+7.33	+1.63
					<i>P</i>	0.0198	$<2 \times 10^{-12}$	0.1030
					Odds: 1 against	49	$>5 \times 10^{11}$	9

¹ Witnessed.

N.B. The probability of a hit is assumed to be $1/5$ in computing the mean and variance.

2. Experimental conditions

M.B., an electronics engineer, and D.C., his assistant, were colleagues of the writer and in 1951 agreed to take part in GESP experiments at our place of work near Bournemouth, in the lunch break. The procedure was similar to but not so rigorous as that followed by Soal and Goldney in their classic experiments with B.S. (3).

Seven out of the nine test sessions took place in A.M.J.M.'s

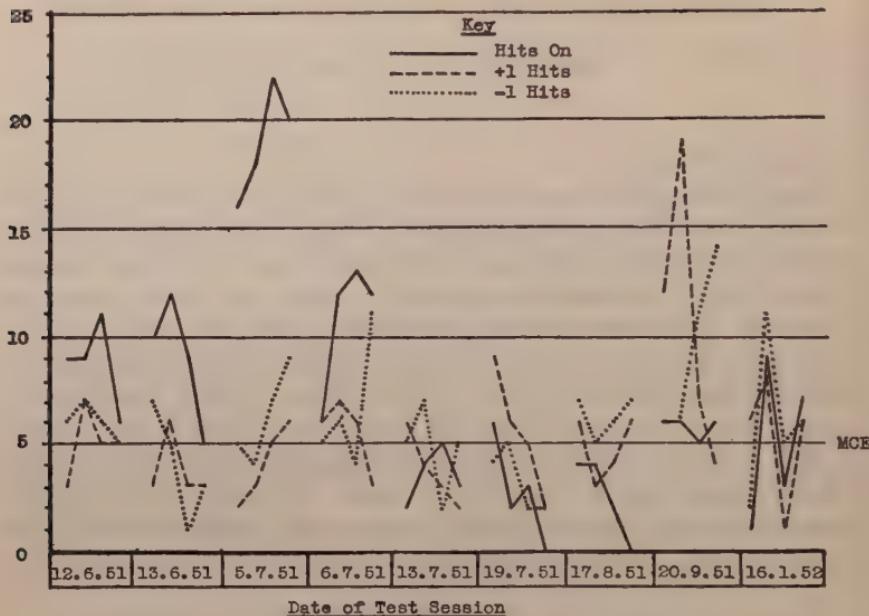
No. of Hits

FIG. 1 : Hits per run of 25 trials for complete test series

laboratory. D.C., with the writer as Experimenter, sat side by side at a table while M.B., with his back to D.C., sat at another table about 12 feet away. A large cupboard behind D.C. screened him visually from M.B. By sitting slightly sideways to the table, A.M.J.M. could look back over his own shoulder round the end of the screening cupboard and keep watch on the activities of M.B. This check could usually be carried out in the short interval between calls. The five ESP cards were handed to D.C. who shuffled them face down and laid them in a row on a sheet of paper. Each card was placed against one of the pairs of numbers 1-0, 2-9, 3-8, 4-7, 5-6, previously written on the sheet. A.M.J.M. then opened a book of random sampling numbers (4) and selected a column of 50 numbers. The successive targets were indicated to D.C. by pointing at the card identified by the number in the column. D.C. lifted the edge of the indicated card and looked at its face while A.M.J.M. called 'right'. M.B. recorded his call on an S.P.R. Scoring Sheet and said 'right' when ready for the next call. This procedure was repeated for a total of 100 calls with a brief pause at the end of each run of 25. The pause was somewhat longer after 50 calls to enable M.B. to continue on a fresh scoring sheet and for A.M.J.M. to transfer to the adjacent column of

random numbers. The average time per call ranged from 4·8 to 13·2 seconds which is several times slower than that for the Soal-Goldney tests with B.S. M.B. stated that sometimes he could not think of any symbol and on several occasions crossed out his first call, replacing it with another before saying 'right'.

At the end of a session A.M.J.M. turned the target cards over, D.C. read out the list of random numbers used, and A.M.J.M., after checking the numbers, recorded the corresponding symbols on M.B.'s score sheet. The sheets were then scored for hits on the target and also for hits on the immediately preceding and succeeding targets (-1 and +1 hits). M.B. was informed of his score and usually a light-hearted discussion ensued. Both M.B. and D.C. were—and still are—sceptical of the evidence for ESP and explained their high scores as being due to some statistical fluke. No strong effort was made by A.M.J.M. to persuade them otherwise. It was not practicable to have all the test sessions witnessed, but observers were present on three occasions during the series.

3. Special test sessions

The scores for the first two sessions were well above chance, and at A.M.J.M.'s suggestion M.B. and D.C. agreed to exchange roles for one session. With D.C. as Perceipient the results were only at chance level and are not reported here. In the next session (5 July 1951) M.B. reverted to his original function as Percipient and the remarkable score of 76 hits out of 100 was obtained. It was agreed to hold the following session in the lounge of a Bournemouth hotel where the Quest Club, a group interested in ESP, held regular meetings. The experimental method was as described above but with M.B. now about 25 feet from D.C., the club members being seated in between. Results were highly significant—although less so than in the previous session—and no *indicia* likely to explain the results were reported by the observers.

The next week (13 July 1951), G. W. Fisk was in the neighbourhood and at his request a test session was conducted in another room of the same hotel—again with club members present. G. W. Fisk provided a list of random numbers for use at this session and acted as main observer. M.B. was now situated about 50 feet from D.C. but otherwise the conditions were similar to those in the previous session. The results were at chance level only. Four further sessions were conducted back in the laboratory with results generally below chance. It was clear that in these sessions the interest of the subjects was on the wane. It

was intended to complete twelve sessions but this proved impossible as M.B. returned to London.

On 17 August 1951 M.B. also guessed the order of four sealed packs of Clock Cards—12 cards per pack. These packs were supplied by G. W. Fisk and returned to him unopened, together with the score sheet. After M.B. had returned to London he agreed to guess another four packs supplied by G. W. Fisk, and the results are given in Appendix II. Better results were obtained with the first packs than with the second but the results are scarcely significant.

4. *Discussion of results*

In view of the high scores obtained, more detailed consideration is now given to the test conditions—particularly with regard to the possibility of sensory leakage between Agent and Percipient. Due to D.C.'s method of peeping at the target card (and the fact that he had previously shuffled them face downwards), A.M.J.M. did not know which symbol was being used for any particular call—until the end of the session. It should also be mentioned that after the first ten trials or so in a session, D.C. did not lift the target cards at all as he stated that he could remember their position in the row. It is therefore improbable that any unconscious clues as to the target were given to M.B. by A.M.J.M. when he said 'right'. Particular care was taken to avoid any change in inflection or timing when A.M.J.M. noted that a target was repeated. Future numbers in the column of random numbers were masked by an opaque card with a corner removed. This card made it easier for A.M.J.M. to take the numbers in order and also prevented D.C. from learning the future target sequence—in case he had some sensory method of transmitting the information to M.B. No such method was detected during these experiments. The columns of random numbers were picked in a haphazard way by opening the book without looking at it. In the writer's opinion it is highly unlikely that the results were due to any conscious fraud on the part of D.C. or M.B.

Turning now to the hits on the actual target, it will be seen from Table 1 that the scores per session rise to a maximum, fall off to below chance, and finally rise to chance level. The fall in score may have been due to the subjects feeling that they were on trial—particularly the session at which G. W. Fisk was present. During later sessions the subjects were losing interest in the tests. It became more difficult to persuade them to participate, and this is reflected in the increasing periods between sessions. Referring to Fig. 1, it will be noted that within each of the first five sessions

the run scores rise to a maximum in the second or third run and then fall in the fourth. This characteristic is not evident in the later sessions, where the tendency is for the scores to decline within the session.

Some attention has been given to displacement hits as there is a remarkably high score in the penultimate session, on both +1 and -1 targets. Unfortunately this session provides inadequate data to test reinforcement effects adequately and as the total result for +1 and -1 hits is not significant no further conclusions can be drawn.

The net result for hits on the target is summarised in Table 2 and the significance calculated by Stevens's method (5) as a check on values derived from the theoretical mean and variance. The significance is somewhat reduced by Stevens's method but still remains extremely high. It will also be noted that the target

TABLE 2
CONTINGENCY TABLE FOR HITS ON—ALL NINE SESSIONS

		TARGET					
		P	W	Sq.	St.	C	Total
C A L L	P	85	56	40	43	40	264
	W	31	37	22	21	26	137
	Sq.	27	33	55	29	28	172
	St.	22	20	17	36	26	121
	C	34	43	37	37	55	206
	Total	199	189	171	166	175	900

Chi-Square for Calls

=72.92 with 4 d.f.

$P < 10^{-6}$

Odds : 1 against,
greater than one million.

Chi-Square for Targets

=4.13 with 4 d.f.

$P = 0.4$

N.B. P = Plus

W = Wavy Lines

Sq. = Square

St. = Star

C = Circle

Significance of total score of
268 hits

Method	$p = 1/5$	Stevens
MCE	180	182.2
Dev.	+88	+85.8
SD	12	14.8
t	7.33	5.80
P	$< 2 \times 10^{-12}$	6.6×10^{-7}
Odds: 1 against	$> 5 \times 10^{11}$	1.5×10^8

symbols were normally distributed but that the calls by M.B. were not. There is a striking number of hits on the Plus symbol, and it seemed worthwhile to see whether the Plus favoured high scoring or whether the effect was merely due to a preponderance

of Plus symbols in the calls. An approximate analysis of the degrees of success with each symbol is given in Appendix I. The results, while not conclusive, suggest that best success was obtained when the Plus symbol was target.

Finally, the results for the first four high-scoring sessions are analysed in Table 3. In this case, Stevens's method and the theoretical method give almost identical results. ($P < 10^{-40}$, Odds extremely high.)

TABLE 3
CONTINGENCY TABLE FOR HITS ON—FIRST FOUR SESSIONS ONLY

		TARGET					
		P	W	Sq.	St.	C	Total
C A L L	P	63	16	12	16	13	120
	W	3	27	6	8	11	55
	Sq.	5	14	38	18	10	85
	St.	2	5	9	26	10	52
	C	10	12	13	17	36	88
	Total	83	74	78	85	80	400

Chi-Square for Calls

$= 38.725$ with 4 d.f.

$P < 10^{-5}$

Odds : 1 against,
greater than 100,000.

Chi-Square for Targets
 $= 0.925$ with 4 d.f.
 $P = 0.9$

Significance of total score of
190 hits

Method	$p = 1/5$	Stevens
MCE	80	80.35
Dev.	+110	+109.65
SD	8	8.003
t	13.75	13.71
P	$< 10^{-40}$	
Odds : 1 against	Extremely high	

5. Conclusions

In nine sessions of 100 trials per session, using ESP cards under adequate (but not stringent) GESP conditions, the Percipient Mr M.B. obtained a total score on the target which was significantly above chance. Mr D.C. acted as Agent and was situated 4 to 17 yards from M.B. The high score was mainly due to an exceptional performance in the first four sessions.

It would clearly be desirable to conduct further tests on these two subjects, but either separate or long-distance tests are indicated since M.B. has moved away.

6. Acknowledgements

The writer wishes to thank Dr D. J. West for advice on the application of Stevens's method and also for a suggestion to examine the degrees of success on the target symbols. Helpful advice was also given by Dr S. G. Soal on this latter problem. Finally, it is a pleasure to acknowledge the co-operation and encouragement of G. W. Fisk during all phases of the work and for permission to quote his Clock Card results with M.B.

REFERENCES

- (1) Fisk, G. W. Home-Testing ESP experiments. *Jnl. S.P.R.*, xxxvi, 1951, 369-70 (Preliminary Report), 518-20 (Second Report).
- (2) West, D. J. Home-Testing ESP experiments: an examination of displacement effects. *Jnl. S.P.R.*, xxxvii, 1953, 14-25.
- (3) Soal, S. G. and Goldney, K. M. Experiments in pre-cognitive telepathy. *Proc. S.P.R.*, xlvii, 1943, 21-150.
- (4) Tippett, L. H. C. *Tracts for Computers*, No. 15, *Random Sampling Numbers*. Cambridge University Press.
- (5) Stevens, W. L. Tests of significance for ESP data. *Psychol. Rev.*, Vol. 46, No. 2, March 1939.

APPENDIX I

DEGREES OF SUCCESS ON THE FIVE TARGET SYMBOLS

1. Introduction

It will be noted that in Table 2, a large number of hits were obtained on the Plus symbol and it therefore seemed worthwhile examining the data more closely to see whether there is a significant difference in scoring rate between the five symbols—particularly as the Plus preponderates in M.B.'s calls. The method given by Soal and Goldney in Appendix A of Reference (3) has been followed. In a private communication, Dr Soal has pointed out that the Chi-Square test for significance may not be strictly legitimate in this instance and may lead to an over-estimate of significance. However, in the absence of a satisfactory alternative it seemed reasonable to quote the results obtained and to leave the question of their precise significance until some future occasion. The calculations have been applied to the complete series of nine sessions and also to the first four high-scoring sessions.

2. Method of Analysis and Application to Data

The notation to be used is tabulated below :

Symbol	P	W	Sq.	St.	C	Total
No of times presented as Target	a_1	a_2	a_3	a_4	a_5	n
No. of times Called	g_1	g_2	g_3	g_4	g_5	n
No. of Hits	s_1	s_2	s_3	s_4	s_5	S
No. of True Cognitions	x_1	x_2	x_3	x_4	x_5	X

The symbols are Plus, Wavy Lines, Square, Star, Circle.

If s_1 hits are obtained on a particular symbol then a certain number can be attributed to chance and the remainder x_1 , to 'true cognitions'. Then,

Where

$$X = x_1 + x_2 + x_3 + x_4 + x_5$$

X is the most probable number of 'true cognitions' and can be found from

Since s_1 , a_1 , g_1 , n and now X are known, x_1 may be found from (1) as the positive root of a quadratic equation. Thus for all nine sessions, we have from Table 2,

Symbol	P	W	Sq.	St.	C	Total
$a_1 \dots a_5$	199	189	171	166	175	900
$g_1 \dots g_5$	264	137	172	121	206	900
$s_1 \dots s_5$	85	37	55	36	55	268

By substituting in (2) to find X , we have

$$268 = X + (900 - X)/5$$

Hence $X = 110$

x_1 is now obtained by substituting in (1)

$$85 = x_1 + (199 - x_1)(264 - x_1)/(900 - 110).$$

Solving this and the four similar quadratics for $x_2 \dots x_5$, we find

x_1	x_2	x_3	x_4	x_5	Total
39.838	7.084	29.463	16.094	17.356	109.835

The total is an excellent agreement with the value 110 calculated above for X . It also provides a check on the assumptions implicit in (1) and (2). These values for x , the 'true cognitions' on each symbol, are to be compared with the expected values assuming equal degrees of success on the five symbols. This latter phrase may be interpreted in the two following ways, viz.

$$\frac{x_1'}{a_1} = \frac{x_2'}{a_2} = \frac{x_3'}{a_3} = \frac{x_4'}{a_4} = \frac{x_5'}{a_5} = \frac{X}{n}$$

◎

$$\frac{x_1'}{g_1} = \frac{x_2'}{g_2} = \frac{x_3'}{g_3} = \frac{x_4'}{g_4} = \frac{x_5'}{g_5} = \frac{X}{n}.$$

Here, the primed values of x are the expected values as opposed to the calculated observed values. Taking the first criterion based on target frequency, we find,

	x_1	x_2	x_3	x_4	x_5	Total
Observed	39.838	7.084	29.463	16.094	17.356	109.835
Expected	24.286	23.065	20.869	20.258	21.357	109.835
Deviation	+15.552	-15.981	+8.594	-4.164	-4.001	

Chi-Square = 26.2 with 4 d.f. $P = <0.001$.

On the call frequency criterion the results are,

	x_1	x_2	x_3	x_4	x_5	Total
Observed	39.838	7.084	29.463	16.094	17.356	109.835
Expected	32.218	16.719	20.991	14.767	25.140	109.835
Deviation	+7.620	-9.635	+8.472	+1.327	-7.784	

Chi-Square = 13.3 with 4 d.f. $P = <0.01$.

These two results for Chi-Square suggest that equal success is not obtained on each symbol—best results are obtained in the Plus, worst on the Wavy Lines.

Considering now the first four sessions only (Table 3), we find,

$$X = 137.5$$

Target frequency criterion

	x_1	x_2	x_3	x_4	x_5	Total
Observed	56.636	19.697	26.540	16.894	20.887	140.654
Expected	29.186	26.021	27.428	29.889	28.131	140.655
Deviation	+27.450	-6.324	-0.888	-12.995	-7.244	

Chi-Square = 34.9 with 4 d.f. $P = <0.001$

Call frequency criterion

	x_1	x_2	x_3	x_4	x_5	Total
Observed	56.636	19.697	26.540	16.894	20.887	140.654
Expected	42.197	19.340	29.889	18.285	30.944	140.655
Deviation	+14.439	+0.357	-3.349	-1.391	-10.057	

Chi-Square = 8.78 with 4 d.f. $P = <0.05$.

It will be noted that Σx is now not so nearly equal to X but the agreement is reasonable. The latter Chi-Square value is scarcely significant but again, best success is shown on the Plus. On the target criterion, worst success occurs for the Star but taking the call criterion the Circle worst.

3. Conclusions

Bearing in mind that the use of the Chi-Square test in the above calculations may over-stress the significance of the results, it is probably only safe to say that equal success was not obtained on each of the five symbols and it seems likely that best results were obtained on the Plus. There is some indication that the symbol with which worse success was obtained varied between the first four sessions and the subsequent five test periods.

APPENDIX II

RESULTS OF DTSP CLOCK CARD TESTS WITH MR M.B.

The test procedure and method of evaluating the results has been adequately described¹ and will not be repeated here. The following Table summarises the results obtained in the two sessions of 48 trials each :

Percipient M.B.

	Date of Session			
	17.8.51		19.7.52	
	Direct Hits	Sum of Divergences	Direct Hits	Sum of Divergences
Total	9	77	2	111
MCE	4	84	4	84
Dev.	+5	+7	-2	-27
t	+2.61	+0.57	-1.04	-2.09
P	0.009	0.57	0.30	0.037
Odds : 1 against	111	—	—	27

These results are of interest in that the score is first positive and then negative but the net score is not significant.

¹ Fisk, G. W. and Mitchell, A. M. J. ESP experiments with Clock Cards. *Jnl. S.P.R.*, Vol. 37, 1953, 1-14.

THE SOCIETY'S NEW PRESIDENT

Professor F. J. M. Stratton, D.S.O., F.R.S., was elected President of the Society on 29 April, in succession to Dr Gilbert Murray. Professor Stratton was Professor of Astrophysics and Director of the Solar Physics Observatory at Cambridge University from 1928 to 1947, and President of Gonville and Caius College from 1945 to 1948. He joined the Society in 1902, and has been a member of the Council since 1947.

REVIEWS

RELIGION, PHILOSOPHY AND PSYCHICAL RESEARCH: selected essays. By C. D. Broad. London, Routledge & Kegan Paul, 1953. vii, 308 pp. 25s.

No reader of this Journal will need to be recommended to a book by Professor Broad, and indeed most of them will have already made acquaintance with the substance of three of the papers in the section dealing with Psychical Research. Nor will anyone familiar with any of Dr Broad's writings need to be told that his latest book is marked throughout by the subtlety of analysis, candour and fairness of judgement, and lucidity of exposition spiced by abundant wit and mordant epigram, which are so characteristic of him as a philosophical assayer of unacknowledged assumptions and reasonings that claim cogency.

The book is divided into three sections, Psychical Research (just over half of the whole), Religion (something under a third), and Politics (a little more than a sixth). This proportion fairly represents also the values of the respective sections. In the first two a number of papers and articles are brought together on overlapping subjects, but there is not in fact much duplication of argument and treatment. Certainly everyone interested in the general subjects of thought covered in this volume will find something specially stimulating in one or other of Dr Broad's discussions.

In the first and most important section two of the papers are connected with famous names, Henry Sidgwick and Immanuel Kant. The account, delivered as a centenary address, of Sidgwick's labour to give psychical research a scientific foundation is a work of *pietas*. Dr Broad's profound respect for Sidgwick as man and as thinker is well known, but here he sets this eminent

Cambridge figure illuminatingly in the context of Victorian contemporaries so as to bring out by comparison Sidgwick's own rare tolerance, patience, persistence in research, and freedom from prejudice. The full discussion of Kant's position, particularly as shown in the *Traüme eines Geistersehers*, was an important service to criticism, for this early work of Kant has been strangely neglected. (I remember my surprise at being told by a distinguished Kantian critic of a generation ago that he had never read it.) Dr Broad gives a careful and detailed analysis of Kant's exposition of the notion of a world of 'spirits', and his own critical comments are masterly. He has taken great trouble also to verify the references to Swedenborg and his alleged supernormal experience, and here Dr Broad's knowledge of Sweden and Swedish (probably unprecedented in a British philosopher) gives his account special authority. One is left feeling that had Kant had available to him the cumulative results of seventy years' careful examination of 'abnormal' phenomena in the psychological field it would have deeply influenced his views about natural science and the human mind.

Certainly he would have appreciated the opening paper of Dr Broad on 'The Relevance of Psychical Research for Philosophy', which ought to be read by every intelligent 'general reader' who has for good or bad reasons ignored the whole subject. Less satisfactory, I have to confess, are some passages in the second paper, the longest in the book, on 'Normal Cognition, Clairvoyance and Telepathy', Dr Broad's Presidential address to the S.P.R. given in 1935. This is a searching examination of the ambiguities and complexities in these paranormal experiences, of which the ordinary user of terms like clairvoyance and telepathy remains in contented ignorance. But Dr Broad concludes with a speculative suggestion in which one reader at any rate finds some difficulty. On p. 43 he rebukes the believer in clairvoyance as a unique faculty, who yet is unable to suggest its *modus operandi*, for postulating a 'something, I know not what' after the manner of Locke. Locke's *Nescioquid* is a familiar missile in philosophical discussions, but is it not applicable to Dr Broad's own concluding speculation also? For there (p. 67) he suggests an unknown Substratum (a word with Lockian echoes), neither mind nor body, but 'some kind of extended pervasive medium, capable of receiving and retaining modifications of local structure or internal motion', in 'some way', I presume, 'we know not how.' I do not myself see why, if we are compelled to use substratum-language at all, we may not speak of a *mental* substrate, when in any case we have to swallow a chastening dose of agnosticism. And even the

Hegelians might stir with a faint protest. (Dr Broad's wide philosophical charity stops, I think, abruptly when he detects the faintest whiff of Hegelianism.) For on p. 111 we read of those who 'contrived to muddle themselves into a kind of Hegelian Christianity, in which everything turned into its opposite, and Materialism and Mentalism were resolved into a higher synthesis in the glow of which one felt it to be crude and ungentlemanly to raise concrete questions about historical events and contemporary phenomena'. Bishop Butler's 'Things are what they are', etc., prefixed to this book, is a wholesome warning, it seems to me, against the assumption of a material-mental substrate as well as against the dogma of a materialist-mentalist synthesis.

Leaving without comment (which I am incompetent to offer) the paper on Dunne's theory of Time, I pass to the second section, on religious themes, the existence of God, the validity of belief in a personal God, Bishop Butler as theologian, and the present relations of Science and Religion. In view of Dr Broad's very frank admission that he has never had any experience that could be called religious, the fairness with which he seeks to appraise religious claims and arguments is indeed humbling to more impatient minds. He makes it clear that he thinks very little of the claims or prospects of Christianity in particular, but his attitude has nothing in common with that of the nineteenth-century freethinker, whether of the gloating or the patronising variety. It is more like the regretful discharge from a firm of a trusted employé of long service, whose character is respected but who has been found to be totally unqualified for the increasingly complex duties he has to perform. Very few completely tone-deaf people (to use his own analogy) can have written, or spoken, about music with any such dispassionate objectivity. Yet many readers will remain unsatisfied. Perhaps they will feel that no set of credal statements, however carefully and fairly given (p. 221), can possibly be free from elements of paradox and ambiguity, seeing that in this matter language, developed for totally different purposes, is being subjected to a strain under which every symbol and analogy bends and buckles. (I wish that Dr Broad had used the comparison with poetry as well as that of music, for the poet, like the theologian, is using conceptual terms and propositional forms, though not in a way that always makes literal coherent sense.) We may perhaps feel also that the brusque dismissal (p. 229) of the plea, virtually that of *credo ut intelligam*, rather misses the point. If religion involves self-committal to a way of life, it would be strange indeed if its significance (including such truth claims as it involves) could be altogether fairly assessed by

anyone who does not make some venture of self-committal. Only so, after all, do we discover the truths implicit in the relation between human friends ; by daring to trust ahead of conclusive evidence of trustworthiness.

I have left myself no space for more than the briefest comment on the third section of the book, the two papers on 'War Thoughts in Peacetime' and 'Some Fallacies of Political Thinking'. The former dates from 1931, and the author has an interesting appendix in which he notes points on which his opinions have been modified ; but he is certainly entitled to underline those parts of his gloomy prognosis which events have corroborated. In both papers unpopular truths, some of them too obvious to be often noticed, are driven home with vigour, and with something else, with occasional manifestations of feelings—indignation and scorn, of which there are few traces in the earlier sections of the book. But in this passion-infected field of politics even Dr Broad has now and then let his feelings warp his cool assessment of facts. It is possible to paint even the Kremlin blacker than it is ; and there is a phrase used of Gandhi (p. 279) which I suspect that no one who knew him would accept for a moment as just.

But I must not conclude on a note of cavil. Let me rather say that this book affords ample evidence, if evidence were needed, that no one has done more than Dr Broad both to commend the serious study and appraisement of Psychical Research among thinking men, including philosophers, and to promote the philosophical criticism and interpretation of their work among those primarily engaged in psychical investigations.

JOHN W. HARVEY

DIVINE HORSEMEN : the living gods of Haiti. By Maya Deren. London, Thames & Hudson, 1953. 350 pp. Illus. 25s.

This is a fascinating, a learned, and an important book, making substantial additions to our knowledge of Voudoun, the complex religion drawn from African, Caribbean, and Mexican sources which has developed among the mixed and poverty-stricken working population of Haiti. To most of us this religion, under the name of Voodoo, has been passed by as an insignificant affair, a matter of confused modern mumbo-jumbo combined with primitive and rather sinister superstition. Miss Deren, who tells us tantalisingly little about herself, except that she went to Haiti primarily to make films of the Voudoun rites and that her father gave her a background of psychiatry, shows a very different picture.

Her competence as an anthropologist and her power of entering sympathetically into the culture which she both shared and studied stand out from every page of her book. She leaves us in no doubt that the Voudoun rites have an immense stabilising and integrating force, and one which is in the main healthy. The *houngan*, who administers them and who embodies the living tradition of their meaning, is normally a person of high character, serving his community well, and even able, by his knowledge, to control the deities, or invisibles, whom his rites invoke.

The essence of Voudoun lies in the manifestation of these deities, now very numerous, though some are more ancient and wider in their influence than others, by possession, a condition in which the deity or *loa* 'mounts the head' of some worshipper or even of some bystander. When this happens the person so possessed completely loses both consciousness and his own personality, that of the *loa* being manifested with striking consistency. Thus the different *loas*, Ghede, Loco, Erzulie and the rest, have come to be recognizable and personal agents. It is difficult to refuse them the rank of actual entities, or to realize how completely they are dependent upon the rites and traditions which call them into activity.

The whole subject is one of profound significance for parapsychology, with its unsolved problems of the real nature of those detached quasi-personalities which are apparent in some cases of hysterical dissociation and, very probably, in some poltergeist phenomena. What is very striking in Voudoun is the strong sense of social responsibility which penetrates the whole system, in the sharpest possible contrast to the wild excesses which made unspeakable havoc in the Witches war, in Europe and at Salem, and to such cases of diabolic possession as those at Loudun, recently described by Aldous Huxley.

No short review is adequate. The book must be read. And I can promise the reader that he will find it hard to put it down.

L. W. GRENSTED

THE JOURNAL OF PARAPSYCHOLOGY. Vol. 17, No. 1, March 1953.
Durham, N.C., Duke University Press. \$1.50.

The first article in this number is a reprint of last year's Myers Memorial Lecture by the present reviewer on 'Psychical Research Past and Present'.

An article on G. N. M. Tyrrell and his work by Salter, Fisk, and Price is a reprint from our Journal.

J. G. Pratt contributes an important survey of work on 'The

Homing Problem in Pigeons' with particular reference to the work of Matthews in Cambridge and of Kramer and St Paul in Germany. The general finding has been that birds taken for long distances become oriented in the homeward direction soon after their release. Pratt argues that the sun orientation hypothesis does not explain the facts that are known at present, and suggests a new line of experimentation with moveable lofts which would provide a more definite criterion of the use of ESP.

'An exploratory study of some variables relating to individual ESP performance' by R. L. Van de Castle is an ingenious experiment to test with a single subject both the effect of varying the time taken in ESP guessing and the nature of the target. He claims to find definite effect of kind of target on the score and a less definite indication of effect of time for guessing. It does not appear, however, that either the types of target or the times were systematically randomized, which makes it possible that spurious differences may have been produced by chronological changes in scoring level. If the experiment is repeated with systematic randomization of the variables, the significance of any observed differences might well be tested by the more sensitive method of the analysis of variance.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 47, No. 2, April 1953. New York, A.S.P.R. \$1.50.

In the January issue the major article was 'The Psychic Fifth Dimension' by Hornell Hart. The April issue contains a further digest of the comments of Mr Hart's correspondents and a further elaboration of the five-dimension theory, which he claims provides a framework within which other psi theories can readily be classified and compared and opens the way to developing a rational conception of the character of life beyond death. He wishes to promote the carrying out of an experiment in the induction of 'shared clear dreams of the personalities of deceased persons, and to apply to such personality-representations appropriate tests of identity such as are used in borderline identity tests in the sensorimotor world, and such as might be developed in the study of reciprocal dreams between living persons'.

A short note states that Mrs Dale carried out a repetition of the Kahn-Neisser experiments (reviewed in the May-June issue of this *Journal*, p. 105) using IBM answer sheets as targets, with the co-operation of Dr B. F. Riess of Hunter College, New York. The total score was not significant and no significant declines

were found. There are also some comments on Kahn's paper by Professor R. A. McConnell of Pittsburgh University.

An interesting and typical case of an apparition seen collectively by six people is contributed by a Polish army officer. The case is also typical in that it was ten years old when brought to the attention of the Society, that no contemporary record was made, and that all five of the other witnesses are now dead. I am hoping that my cynicism may one day be confounded by the publication of a spontaneous case with adequate documentary evidence which leaves no chink for the wedge of counter-hypotheses.

DENYS PARSONS

CORRESPONDENCE

'SURVIVAL AND THE IDEA OF "ANOTHER "WORLD"'

SIR,—I am much indebted to Mr A. G. N. Flew and Dr J. R. Smythies for their very interesting criticisms of my lecture on 'Survival and the Idea of "Another World"', *Journal* March-April 1953, pp. 79-80, and May-June 1953, pp. 109-12.

I agree with Mr Flew that one needs to show that 'we' might become such beings after death [i.e. beings who experience an image-world], that death for *us* might be a metamorphosis from a substantial to an insubstantial mode of existence'. He says, further, 'it would be necessary to show that it would be reasonable, if certain conditions were satisfied, to *decide* that particular incorporeal beings could be identified with, could be said to be the same persons as, particular human beings.' I agree with him that this would be at least partly a matter for terminological decision. And even though the conditions in question were fulfilled, someone might still prefer to say that a particular image-experiencer is *not* 'the same person as' the late Mr Smith. He might decide to say, instead, that this image-experiencer is 'linked with' the late Mr Smith in such and such ways, or is 'the *post mortem* successor' of the late Mr Smith. So long as we can find some fairly unambiguous way of tying him on, so to speak, to the late Mr Smith, it will not matter so very much whether or not we describe this tie in personal-identity terminology. I am inclined to think (though Mr Flew will probably not agree) that even in this present life personal identity is a matter of degree, and not a matter of all or none.

The tie or link which we should have to look for would obviously

be concerned in one way or another with memory. If our image-experient was able to recollect a considerable number of *ante mortem* experiences which admittedly belonged to Mr Smith and to him alone, that would go a long way towards establishing the required tie or link. But I do not think we need to demand as much as this. The word 'memory' covers much more than a capacity for explicit recollection. If the images which our discarnate experient is aware of are correlated with the late Mr Smith's percepts in the same kind of way that images I now have (e.g. when dreaming) are correlated with earlier percepts of mine, we should be inclined to say that they are memory-images 'of' some of the late Mr Smith's experiences. If it were also found that the contents of our discarnate experient's image-world were arranged in such a way as to fulfil wishes (conscious or repressed) which were characteristic of the late Mr Smith when he was alive, the link we are trying to establish would be strengthened further. Whether we elected to use the language of personal identity or not, we should then be inclined to say that *something* of the late Mr Smith has persisted, or that the image-experiences of this discarnate experient are in some (important) way 'continuations of' the experiences which made up the mental history of the late Mr Smith. In actual fact, if these conditions about memory-linkage were fulfilled, most of us probably *would* decide to apply the language of personal identity and would have few qualms about doing so. But though in the case supposed we should have strong grounds for taking that decision, it certainly would involve some modification of the rules we ordinarily use for applying personal identity language. The sort of continuity which we should have established between a certain discarnate experient, on the one hand, and the late Mr Smith on the other, certainly would differ in some respects from the sort of continuity there is when the living Mr Smith at the age of 40 is said to be the same person as he was at the age of 30.

It seems to me, however, that Mr Flew underestimates the difficulties there sometimes are about establishing personal identity even in this life. And just for that reason, I think, he *over-estimates* the difficulties of establishing personal identity between this life and the next. As I have said already, personal identity even in this life seems to be a matter of degree. (Is the author of these remarks *just* the same person as the schoolboy who started to learn Latin grammar some 45 years ago?) Moreover, even in this life we use two different sorts of criteria for it, and sometimes the two sorts of criteria conflict. This brings me to Mr Flew's example about the man who robbed the bank. He says 'A

person either is or is not the one who robbed the bank ; he cannot be one thing *for himself* and another *for other people*' (p. 80, Mr Flew's italics). But I think there might be circumstances in which we should say that this man is in one sense the person who robbed the bank, and yet in another sense he is not. Suppose that, like the Rev. Ansel Bourne,¹ he suffers from the disease called Alternating Personality. Other people saw him entering the bank last Tuesday night. They saw him coming out again half an hour later, and his pockets were stuffed with banknotes. But he himself can remember absolutely nothing of what happened between 12 noon on Monday and 12 noon on Wednesday. When taken to the bank strong room, he does not recognise the place at all, and it appears to him utterly unfamiliar. Shall we say he is the person who robbed the bank? Certainly this human organism which now stands in the dock is the same as the one which entered the bank, and these hands are the same as the ones which abstracted the banknotes from the safe. But we should be inclined to say that in such a case our ordinary rules for the use of the phrase 'same person' had broken down. Ordinarily, when the external or observational criteria for personal identity are fulfilled ; the internal or memory criteria are fulfilled as well. But here is a case where this usual concomitance no longer holds. The external criteria are fulfilled, but the memory criteria are not. In the rather clumsy language I was using in the lecture, for others he is the person who robbed the bank, but for himself he is not. Psychologists, however, have invented a special terminology for describing such cases. They have introduced the term 'personality', as distinct from 'person'. They say that this is the same human organism as the one which entered the bank and abstracted the banknotes, but the 'personality' now manifested by it is different from the personality which was manifested by it at the period when these pieces of bodily behaviour occurred. It may be noted that we are driven to say the same kind of thing about trance-mediumship. The organism is the same one during the trance as it is at other times. But the utterances and other behaviour which occur during the trance, at any rate during 'deep' trance, manifest a secondary personality, quite different from the one which is manifested by the organism's ordinary waking behaviour.

In these cases, the bodily criteria for using the phrase 'same person' are fulfilled, but the memory criteria are not. In the hypothetical case which concerns us when we are discussing *trans mortem* personal identity, it is the other way round. The

¹ cf. William James, *Principles of Psychology*, Vol. 1, pp. 391-3.

memory criteria are supposed to be fulfilled, but the bodily criteria are not. Even then, however, there might be something rather *like* what happens when the bodily criteria are fulfilled. Even in this life, a telepathic phantasm closely resembling A's body may be experienced by another person B. Something similar might happen in the 'other' world as I was conceiving it. A discarnate experient X might cause another discarnate experient Y to experience a set of mental images resembling the late Mr Smith, and X might always do this whenever he was in telepathic communication with Y. In that case, if the memory-criteria for linking X with Mr Smith's *ante mortem* mental history were fulfilled as well, I think Y might quite excusably say that he was 'meeting' Mr Smith; and Mr Flew's principle that 'people are what you meet' would not be entirely without application.

I now turn to the criticisms made by Dr J. R. Smythies (*Journal* May-June 1953, pp. 109-12). Dr Smythies's main objection is that I have made the division into the two worlds at the wrong place. Sensa and images, he says, are in the same space, and not in different ones as I had suggested; the real division is between sensum-and-image space on the one hand, and the space of the physical world on the other. I am afraid I did not express myself at all clearly. That was partly from a desire to avoid technicalities as far as possible. But let us consider the difference between normal waking perception and dreaming. I think it will not matter to my argument if the two can sometimes occur together (cf. the last paragraph of Dr Smythies's letter, p. 112). According to the theory of perception which Dr Smythies adopts, waking perception always includes the experiencing of sensa. But surely this is not *all* that it includes. In addition, the sensa which one experiences are referred to objects. The colour-expanses, tactual pressures etc. which one experiences are taken to be in some way constituents or manifestations of ordinary three-dimensional material things. Moreover, a number of *different* sensa sensed on different occasions—visual and tactual ones, for instance, or visual sensa sensed from different points of view—are often referred to the *same* material object. Similarly with dreaming. We do not just experience, say, a rectangular pinkish-white image. In addition, we *refer* that image to some object, a house for instance. And we refer different images to the *same* object. If I asked you to describe your dream to me, you would say that you dreamed of a house; that you saw it from various points of view, went into it, walked about the rooms, and so on.

Now in the distinction I draw between different spaces, I was not concerned with the relations between the sensa and the images

themselves, considered just as two sorts of immediately-experienced particulars. I was distinguishing between the *objects* to which sensa are referred, and the *objects* to which images are referred; and I was suggesting that these two sorts of objects, though similar in many ways, could not be in the same space. For example, the tiger to which I refer my dream images is not anywhere in relation to the college quadrangle to which I referred certain waking sensa just before I went to bed. It is of course somewhere in relation to the palm trees which I dream of at the same time; but they again are not anywhere in relation to the quadrangle.

The point may be put in another way if we prefer not to use the words 'sensum' and 'image' as substantives (a practice which many contemporary philosophers dislike). If we please, we may use a terminology of 'appearing' instead. We then proceed as follows: in waking perception there *sensibly* appear to us to be objects of various kinds—houses, trees, cats, clouds etc. In dreaming, likewise, there *imaginily* appear to us to be objects, very similar to the waking ones in various ways (e.g. extended, shaped, located, coloured). And then the contention is that the objects which there appear to be in the one case and the objects there appear to be in the other cannot be in the same space, though both alike are spatially-extended entities.

I am afraid I may have misled Dr Smythies here by talking about 'an image world'. When I said that the next world might be conceived as an image world, I meant that in after-death experience images might play the same epistemological part as sensa do in our life in this world. (Among them, no doubt, there would be 'somatic' images, resembling the 'somatic' sensa by means of which we perceive our own bodies in this present life.) Moreover, if sensa had altogether ceased to occur, so that there was no longer anything with which images could be contrasted, we should have the same evidence for the *reality* of the objects to which images are referred, as we now have for the reality of the objects to which sensa are referred. We certainly should have, if we interpreted the phrase 'real object' in a Phenomenalistic manner, to mean 'a system of appearances actual and possible'. For we should have good ground, in the next life, for thinking that there were systems of actual and possible images. It might be, of course, that the Phenomenalistic theory, even though true of the next life, was false of this one. A Realist theory, something like Locke's, might be true of *this* world, as Dr Smythies thinks it is. But if it should turn out to be so, I do not think we need be greatly worried. A Phenomenalistic next world will serve all the

purposes for which a next world is postulated. And as I tried to show in the lecture, it need not be a wholly private world, if telepathy is allowed for. It might be public to a number of experiencers whose memories and desires are sufficiently similar.

H. H. PRICE

New College, Oxford.

SIR,—May I offer a few observations on Professor Price's 'Survival and the Idea of "Another World"' (*Proc. S.P.R.*, Vol. 50, Part 182) and Mr Flew's letter (*Journal*, Vol. 37, No. 674)?

The plausibility of Professor Price's speculative world in which man might survive is a stiff challenge to sceptics. It would have to be, he postulates, the world of our thoughts, since we should have lost our body and its sense-percepts. If, then, we can prove that recognizably personal thought, initiatory as well as reminiscent, survives bodily death, then we have proved all that matters of survival. What other personality have I, when 'in the body'—am I ever *in it*?—than the heterogeneous concatenation of percepts—origin and mechanism so far unexplained—which I call my thoughts or consciousness?

In actual fact, a large proportion of our ante-mortem life already passes in a world of thought, or at least of indifference to, or oblivion from sense-percepts. With some the proportion is very large; at certain times it may be large with even the most sensual. Apart from spending a third of the twenty-four hours asleep and dreaming, most of us day-dream, some are hypnotized, anaesthetized, knocked unconscious, go into trance, somnambulate. Often much of this thought life is 'unreal', 'illogical': my thoughts hop, flow, fly, in ways and with rapidity impossible to my body. Are they any less *I*? Surely at the moment of thinking, they are the only *I* that I am aware of? Is the earth-existence of the physical body the sole *raison d'être* of thoughts which seem so often independent of it? If they are independent in its lifetime, might they not continue so afterwards?

The mere fact of dreaming shows that I have, even while 'alive', some sort of 'life' which has no other connection with my physical body than that the *I* who experience the dream am apparently the same as the *I* who remember it after waking. I therefore have, even in my body's lifetime, an existence in which it has no direct part, but in which *I* certainly have. A very active thought-life goes on, too, in paralyzed people who show little or no sign of 'life'. Is it really so daring to suppose that the *I* who thinks continues to

exist when the physical organism ceases even to tick over in sleep and other deathlike states?

Which is primordial during physical life—my thought (whatever that may be), or activity in my physical cerebral cortex? Or are they—blessed phrase!—in non-causal synchronicity? It is as legitimate to postulate 'thought' autonomous as to postulate it the product of chance or sense-stimulated activity in the cortex. Neither has yet been 'scientifically' proved. It is not true anyway that all thought is sensory in origin. Very interesting thoughts come when I am in bed but not asleep: I see, hear, smell, taste nothing, I am oblivious to the sound of my breathing, the smell and contact of the bedclothes, the flavour of my toothpaste. I have some of my most important thoughts when my senses are quiescent; a good case for saying that the physical senses *inhibit* thought rather than originating it.

If the mechanist answers that these thoughts are really sensory reactions once or more times removed in memory, how does he explain memory, or prove it to be physical? What in my physical make-up stores vast numbers of sense-percepts and delivers them hours or years later at will, or even involuntarily? What physical mechanism gives some the power, beyond ordinary 'foresight' or deduction, to see what has not yet taken place? A much larger part of our earth life than we usually realize is already such stuff as dreams are made on.

Though Professor Price discards the word 'unreal', he too readily, I think, assumes a 'dreamlike' post-mortem existence, and Mr Flew talks of 'incorporeal beings'. Surely it is our memory of 'what we meet' in dreams which seems 'dreamlike'. If I could stay in my dream and not see it through the decontamination-process of waking up, I should no doubt be satisfied with the 'reality' of 'what I meet'; they (people and things) would necessarily be of the same order as the dreamer and maybe as mutually impenetrable or exclusive as entities in the physical world. Only those in affinity with us or within our 'wave-band' could enter our next world. But that already operates, rather imperfectly, in this one: only a few of the thousands I 'meet' enter with my permission into my thought-world (those I find congenial), while the rest I exclude or put up with as briefly as possible. Our dream-world has a 'dreamlike' quality when somehow our physical (?) apparatus remembers it, as a 'ghost programme' on the radio sometimes intrudes on another at ordinary strength. But 'dreamlikeness' is not necessarily an essential trait. Perhaps if we could retune our set, the 'ghost programme' would come in at normal strength.

Though carefully disclaiming any assumption as to 'survival', Professor Price has followed Euclid's method with awkward propositions: 'If such-and-such were true, then so-and-so would follow; but so-and-so is true, therefore such-and-such....' In another place (*Light*, May 1953) I have suggested that the onus is not necessarily on those who accept the survival hypothesis to prove it, but that those who disbelieve should produce disproof: it would at least be a useful mental exercise, from which they are at present precluded by their assumption that the onus is on the other party.

Mr Flew's point 4 (that 'spirits' who had 'in the body' the richest image life should be expected to have a more vivid life 'in the next world', and that evidence of this might be a 'useful research pointer') is very important, but quite 'unscientific' because (a) we can have no 'scientific' proof of life, vivid or otherwise, in an unsensory next world, (b) any evaluation of such evidence would depend on the testimony of surviving friends. In a word, Mr Flew is asking for recognition of *qualitative* evidence, which orthodox science will have none of. There is already a greater *quantity* of evidence for survival than has been necessary to secure acceptance of many other hypotheses less revolting to 'scientific' minds. Perhaps we also have adequate qualitative evidence. But to assess it we must have qualitative minds and qualitative standards of measurement. These simply do not exist unless we lay aside our quantitative conventions and use other measuring faculties we possess but rigorously exclude from our 'science'. Are they really less reliable than those on which scientific laws are based? After all, scientists are often wrong, the 'laws' of one generation are scrapped or modified by the next. Our senses are constantly deceived, yet scientists will still accept no other arbiter. They can measure dimension (by arbitrary scales), mass, specific heat (relative to water), but are soon at sea when it comes to sound ('musical', 'unmusical', 'dull', 'loud'), colour (I have no idea whether your 'blue' is the same as mine: when you say it is light of a certain frequency, this means nothing to my senses), smell, taste ('sweet', 'bitter' are entirely subjective words, and quite unscientific). There are no units for measuring these qualities which science uses in its 'precise' lists of properties. When one sound (called a word) has meanings varying with the *tone* or *context*, science can do nothing about it. Still less can it deal with 'truth', 'beauty', 'love'. Yet no one can deny these sounds express *realities*, at least in our *thought-world*.

Whatever tells us the differences between them (and myriads of other sounds, smells, sights, tastes, touches) is quite 'unscientific'

because qualitative, not quantitative. What hope is there of getting minds conditioned by traditional science to accept qualitative evidence?

V. P. UNDERWOOD

Chertsey, Surrey.

PROBABILITY AND PARAPSYCHOLOGY

SIR,—In a broadcast in the B.B.C. Home Service 'Is there anything in it? (Telepathy)' on 21 April, Mr G. Spencer Brown made a peculiar criticism of the experimental work on extra-sensory perception. He claimed that all that has been established by such experiments is that the mathematical theory of probability, though apparently 'accurate' when applied to other material, is not accurate when applied to the material used in such experiments, e.g. playing cards or dice. Dr West, in answering this argument, referred to 'cross-checks', but did not have time to explain fully the nature and significance of these.

Consider for example the experiments carried out by Dr Soal and Mrs Goldney with Mr Shackleton (*Proc. S.P.R.*, Vol. XLVII). An empirical cross-check was made for each score-sheet. The subject's guesses, in each of the two runs recorded on the score sheet, were scored not only against the order of the target-cards he had been trying to guess, but also against the order of the target-cards for the other run. Now in a total of over 11,000 trials, Shackleton's scores on the set of cards that he had been aiming at averaged nearly 7 right out of 25 (giving odds against chance of more than 10^{35} to 1), whereas in the cross-checks, i.e. when his guesses were scored against cards he had not been aiming at, the results consistently averaged 5 right out of 25 (i.e. the level predicted by the calculus of probability).

It would be frivolous to say that these experiments show that the calculus of probability does not apply to the material employed (animal cards, counters, etc.). The cross-checks confirmed the applicability of the calculus to this material, so Mr Brown would be reduced to saying that the calculus is not applicable for correlating the order of guesses with the order of cards *when the guesses in question are 'aimed' at the cards in question!* But what sort of an explanation would this be? Unless Mr Brown is prepared not only to question the range of applicability of the calculus of probability, but also to reject what logicians call the (experimental) Method of Difference, he must admit that these experiments showed that Shackleton's high scoring was causally connected with a psychological variable (i.e. whether or not he 'aims' at, tries to

guess, the cards in question). The experimental conditions preclude our explaining these facts in terms of any laws or theories now accepted by physicists, biologists, or psychologists. Presumably Mr Brown agrees on this, or he would not be resorting to his eccentric explanation.

In view of this, it is not at all clear to me what Mr Brown is trying to say, and I want to ask him to explain his thesis by telling us exactly how he would apply it to Shackleton's results. There are, of course, several features of these results besides those mentioned above to which Mr Brown's explanation will have to apply; notably Shackleton's consistency (i) in getting significant scores with some agents and chance scores with others, (ii) in getting chance scores when an otherwise successful agent did not look at the target-cards, and (iii) in getting significant scores on the (+1) card-position at the normal rate of guessing and on the (+2) position at the rapid rate.

C. W. K. MUNDLE

Department of Philosophy,
University College, Dundee.

ESP, SPACE, AND TIME

SIR,—In the March–April number of this year's journal, Mr Denis Chesters asks what evidence there is that telepathy is in no way dependent upon the distance between agent and percipient. The answer is, of course, that there is no real evidence whatever.

There may be a few valid experiments which tend to suggest that telepathy does not 'weaken' across the infinitesimal space at our disposal upon the surface of this planet; but no one can possibly know what would be the result of a telepathic experiment carried out across a distance of a few hundred thousand light-years. Even if such an experiment could be carried out, it would be impossible to demonstrate that telepathy is 'independent of space' unless the experimenters had infinite space at their disposal; there might be a threshold of perception, as in sensory perception.

The truth is that ESP's vaunted power to 'transcend space-time' is an amiable and quite unproved dogma of psychical researchers. The facts, slender as they are, point in the opposite direction. For instance, it is extremely difficult—almost impossible—to make an *initial* telepathic rapport across any considerable distance. In the realm of precognition, it appears that those dreams which are fulfilled within a *shorter* period out-

number those which are fulfilled within a longer period. This does not suggest that precognition 'transcends time'.

BASIL SMITH

London, S.W.12.

THE INTERPRETATION OF EXPERIMENTS

SIR,—The interpretation of scientific experiments rests invariably on a set of hypotheses. Scientists may therefore hold different opinions as to the acceptability of alternative plausible hypotheses provided the alternatives are equally compatible with the experimental situation. The situation changes drastically if the available evidence contra-indicates one of the hypotheses. Anyone who then still upholds such a repudiated hypothesis is either guilty of incompetence or of severe bias. It is sometimes assumed that the attitude of scientists is one of strict 'objectivity'. Yet the very fact that some 'rationalists' will cling desperately to experimentally discarded hypotheses suggests that the ideal of impartiality is rarely attained. Nevertheless, the average scientist and particularly the more enlightened minds will prefer to be guided by the experimental situation rather than by preconceived arguments. But it must be realized that the degree of irrationality of severely biased scientists can be high. A drastic example of this is provided in the field of Physics by Professor P. Lennard, who repudiated the theory of relativity on the grounds of the non-Aryan descent of its originator Albert Einstein. Lennard, a Nobel prize winner, provides a vivid illustration of the fact that irrationality can play havoc with the minds of men from whom 'objectivity' might otherwise be expected. It is the purpose of this letter to suggest that a similar degree of bias is shown in recent book reviews by Dr E. J. Dingwall in the journal *Nature*.

Dr Dingwall has recently reviewed a new book by Dr J. Hettinger and a volume entitled *The Psychology of the Occult* by D. H. Rawcliffe. Both books have abandoned the scientific method. Dr Hettinger maintains that further supporting experiments on psycho-kinesis would in his view invalidate the fundamental hypotheses of Statistics. This represents a drastic departure from empiricism. With the same justification people might have argued in 1926 that the validity of Quantum Mechanics would in their view invalidate the axioms of the geometry of Hilbert space. Dr Hettinger's argument is on par with Lennard's doctrine. Four months before Dr Dingwall's review appeared in *Nature*, Dr West had lucidly expounded various statistical fallacies in Hettinger's second chapter. It is therefore surprising

that this repudiation of scientific method should be selected *without comment* by Dr Dingwall and represented to us as though the reviewer agreed with the arguments. It is, I believe, significant that out of the many chapters available Dr Dingwall has carefully chosen precisely those parts which attack in a derogatory manner the work of serious experimenters. Dr Dingwall conveys the distinct feeling that he, the reviewer, agrees with Hettinger's contentions. Yet Dingwall must have known that the better type of experiment by Rhine and his school and the eminent researches of Soal and Tyrrell were in no way invalidated by Hettinger's ineffectual criticisms, which rest on a serious misunderstanding of statistical methods. This suggests either that Dr Dingwall is not as familiar with the statistical arguments as one might expect of a reviewer in *Nature* or that some more personal motives were at work. Such personal motives were operating in the case of Lennard. Again, in the case of Academician Lysenko, as is well known, other than experimental facts have determined scientific pronouncements. In the case of Dr Dingwall this suspicion is not in the least alleviated when we consider his review in *Nature* of Rawcliffe's book.

Mr Rawcliffe pretends to have given us an 'expert' version of the achievements of modern parapsychology. Unfortunately there is hardly a page in his book which does not contain either an important omission of some most relevant fact or a distortion of the situation or some other misinterpretation. It is history repeating itself. In 1910 Miss Amy Tanner published a book in which, as Tyrrell puts it (*The Personality of Man*, p. 236), she 'wanders in a maze of irrationality.... Dr Hyslop found in 27 incidents 148 mis-statements and a host of omissions of important particulars, while, he says, she was silent on 38 incidents more significant than any she treated in her fashion. Yet Dr Stanley Hall, blind to all this, wrote an introduction to her book and called it a "searching, impartial, critical estimate"'. Professor S. Hall was no lesser man than the President of Clark University.

Mr Rawcliffe would like to prove that experimenters in parapsychology form a species of their own : they are all incompetent and lack that rare acumen which only the author of *The Psychology of the Occult* possesses. It would therefore have been most advisable for him to be very meticulous in the presentation of his own material, rather than follow in Miss Tanner's footsteps. For instance, he accused Soal of lack of proper precautions in preventing 'unconscious whispers' from having any effect. Yet he does not state that when both doors were locked in the Shackleton experiments some of the highest and most significant scores were

obtained. Again, there is no mention of Rhine's long-distance experiments. Finally, the Stribic-Martin experiments could not conceivably be explained by means of the 'double-whisper' hypothesis. Yet Rawcliffe omitted to mention this experimental series. It is also extraordinary that there is no mention of Tyrrell's highly significant sessions with Miss Johnson, in which again 'double whispers' were excluded in the later sessions. Rawcliffe suppressed the important conclusions of the Hope Committee in the Rudi Schneider sessions. He only cites Besterman's negative findings, but does not state that Schneider's powers were then weakening and that at some of the earlier London sessions Schneider managed to interrupt an infra-red beam as he had done in Paris. Again, on p. 259 Rawcliffe informs us that 'no case at all can be made out for the *sudden* emergence of psychosomatic lesions through suggestion or hysteria'. Anyone who consults Dunbar's textbook (*Emotions and Bodily Changes*, Columbia University Press, 1947) will see that this is patently untrue. The list of misrepresentations and misinterpretations could be multiplied so as to fill a volume almost the size of Rawcliffe's own book. In many cases where I have attempted to check up on the original sources I have discovered a distorted reproduction. Yet Dr Dingwall wishes to suggest to us in his review that this concoction of bias, error, faulty judgement and blind polemic against experienced and careful experimenters is a 'useful handbook'. He mentions the 'double-whisper' hypothesis as though it were a novel and profound idea. Dr Dingwall should have realized how untenable this concept is and that experimenters have known for nearly twenty years of such possibilities and have taken proper precautions. In any case, even if we are only prepared to accept the best series of experiments, as being unimpeachable by any reasonable criticism, then the case for ESP is fully established. If we tried to show that sometimes people had outstanding mathematical ability, then it would be adequate to produce a single Newton or a single Gauss. It would be no argument to say that most people in any generation are at best mediocre mathematicians. Outstanding mathematical or musical ability is probably as rare as outstanding ESP scoring ability.

We must then conclude either that Dr Dingwall is no longer familiar with the experimental situation since about 1935, or that he has purposely chosen irrational arguments of others as a screen behind which he intends to discredit parapsychological research. It is regrettable that the scientific public should be presented with reviews which tend to give a distorted account of the present position in parapsychology. It would be more courageous for

one who feels that serious criticism of parapsychological experiment is needed to express his opinions openly in the specialist press. Such criticism could then be carefully examined.

G. D. WASSERMANN

Department of Mathematics,
King's College, Newcastle-upon-Tyne.

LIST OF CONTENTS OF 'JOURNAL'

A leaflet has been printed giving the principal contents of every issue of the *Journal* from September 1949, when it became available to the public. Prices, both to members and to the public, are given in sterling and in dollars. The list may be obtained from the Secretary of the Society.